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THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL:

EXHIBITING

A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

IN

MEDICINE, SURGERY, AND PHARMACY.

BRISTOL GENERAL HOSPITAL

VOLUME FIFTY-FIRST.

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Vol. 50, p. 535, line 6, *delete* "the os and cervix."

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A System of Practical Surgery with numerous explanatory Plates, the drawings after Nature. By John Lizars, Professor of Surgery to the Royal College of Surgeons, and lately Senior Operating Surgeon to the Royal Infirmary of Edinburgh. Part I. Edinburgh, 1838. 8vo. Pp. 220.

Anatomical Tables; containing concise descriptions of the Muscles and Ligaments, Fasciæ, Blood-vessels, and Nerves. Intended for the use of Students. By Thomas Nunnelly, Lecturer on Anatomy and Physiology in the Leeds School of Medicine, &c. London, 1838. 12mo. Pp. 240.

A Brief Account of the Application and uses of the Utero-abdominal Supporter, a new Instrument for the Relief and Cure of Procidentia and Prolapsus Uteri, invented by A. G. Hull, M. D. London, 1838. 12mo. Pp. 12.

Observations on Professor Hamilton's Deviations from the ordinary mode of stating Practical Results. By Robert Collins, M. D. late Master of the Dublin Lying-in Hospital. Dublin, 1838. 8vo. Pp. 27.

Observations on Sciatica and other Neuralgic Affections, with a consideration of their frequent origin from a disordered state of the stomach; interspersed with some novel views respecting the origin and prevention of Gall-stones; both diseases exemplified by the author's own case; together with an account of the waters of Bagneres de Bigorre, and Baréges in their Treatment. By Richard Carmichael, M. R. I. A., &c. Dublin, 1838. 8vo. Pp. 38.

An Essay on Food, in which the received doctrine of Modern Physiologists, respecting the Waste of the Body is exploded, the cause of Animal Heat is explained upon New principles, the source whence Nitrogen is derived by Herbivorous animals is established, general rules for the preservation of health are laid down, and the wisdom of the Divine Economy in all is vindicated. By W. Grisenthwaite. London, 1838. 12mo. Pp. 119.

Thirty-ninth Annual Report of the Dundee Royal Infirmary. 1837, 1838. 4to. Pp. 25.

Chemistry of Organic Bodies. Vegetables. By Thomas Thomson, M. D. Regius Professor of Chemistry in the University of Glasgow. London, 1838. 8vo. Pp. 1076.

Diet and Regimen, Physical, Intellectual, and Moral, as means in the prevention and cure of Disease. By Robert Dick, M. D. Glasgow, Edinburgh, and London, 1838. 12mo. Pp. 386.

A Treatise on the Structure, Economy, and Diseases of the Ear; being the Essay for which the Fothergillian Gold Medal was awarded by the Medical Society of London. By George Pilcher, Lecturer on Anatomy and Surgery at the Theatre, Webb Street, Borough. London, 1838. 8vo. Pp. 324.

Practical and Surgical Anatomy, by W. J. Erasmus Wilson, Lecturer on Practical and Surgical Anatomy and Physiology. Illustrated with fifty Engravings on Wood, by Bagg. London, 1838. 12mo. Pp. 492.

On the Improvement of Medicine. The Oration delivered before the Medical Society of London, at their sixty-fifth Anniversary, March 8th 1838. By Theophilus Thomson, M. D. Physician to the Northern Dispensary, and to the Artists' Fund, &c. London, 1838. 8vo. Pp 23.

A Treatise on Neuralgia. By Richard Rowland, M. D., &c. London, 1838. 8vo. Pp. 173.

An Examination of Phrenology, in two Lectures, delivered to the Students of the Columbia College, District of Columbia, February 1837. By Thomas Sewall, M. D. Professor of Anatomy and Physiology. London, 1838. 12mo. Pp. 69.

Observations on the Oriental Plague, and on Quarantines as a means of arresting its Progress. Addressed to the British Association of Science, assembled at Newcastle in August 1838. By John Bowring. Edinburgh, 1838. 8vo. Pp. 45.

A Treatise on Physical Education, specially adapted to Young Ladies. By A. M. Bureaud Riofrey, M. D. &c. &c. Second Edition, considerably enlarged, with Plates. London, 1838. 8vo. Pp. 574.

On the Objects and Mutual Relations of the Medical Sciences; an Introductory Address delivered at the Middlesex Hospital School of Medicine, 2d October 1838, at the opening of the Winter Session. By Frederic S. Leighton, M. D. &c. London, 1838. 8vo. Pp. 51.

Outlines of Military Surgery. By Sir George Ballingall, M. D. F. R. S. E. &c. Second Edition. Edinburgh, 1838. 8vo. Pp. 542.

Anatomie Comparée du Systeme Nerveux, considerée dans ses Rapports avec Intelligence, &c. Par Fr. Leuret, Medecin de l'Hospice de Bicetre. Ouvrage accompágné d'une Atlas de 33 Planches en Folio. 1ere Livraison. Tome Premier. Paris, 1839. 8vo. Pp. 240.

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The London Flora, containing a concise description of the Phænogamous British Plants which grow spontaneously in the vicinity of the Metropolis, with their localities; arranged in conformity to the Natural System, &c. By Alexander Irvine of Marischal College Aberdeen. London, 1838. 12mo. Pp. 340.

The Elements of Materia Medica, comprehending the Natural History, Preparation, Properties, Composition, Effects, and Uses of Medicines. Part I. containing the General Action and Classification of Medicines, and the Mineral Materia Medica. By Jonathan Pereira, F. R. S. and L. S. &c. London, 1839. 8vo. Pp. 559.

On Granular Degeneration of the Kidnies, and its connexion with Dropsy, Inflammations, and other Diseases. By Robert Christison, M. D. F. R. S. E. President of the Royal College of Physicians of Edinburgh, Professor of Materia Medica, and one of the Professors of Clinical Medicine in the University of Edinburgh, &c. Edinburgh, 1839. 8vo. Pp. 288.

London Medical Gazette for the Session 1838–39, New Series. No. 52. (564.) Sept. 22d to No. 11, (575.) December 8th.

Comptes Rendus Hebdomadaires des Séances de l'Academie des Sciences, par MM. Les Secretaires Perpetuels. No. 8, 20th Aug. No. 22. 26th November 1838.

The American Journal of the Medical Sciences, No. 43, May 1838, and No. 44, August 1838.

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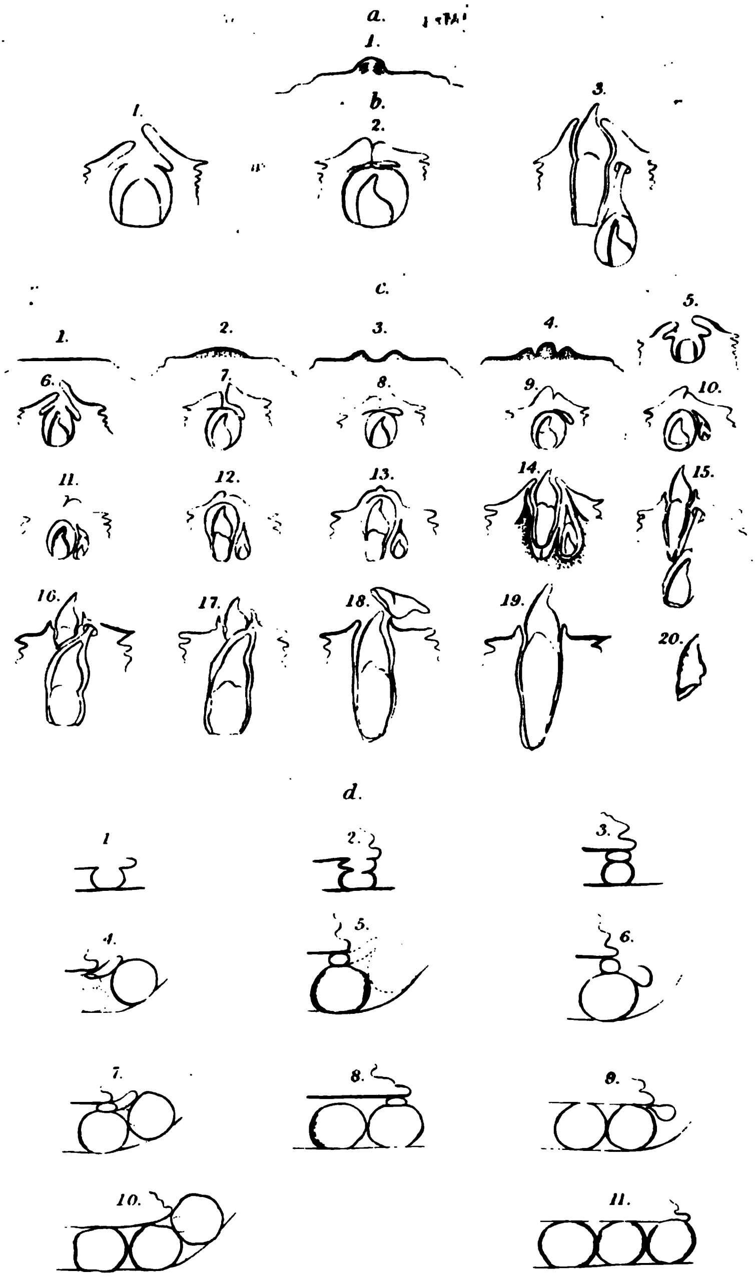
Mr GOODSIR on the Origin and Developement of the Pulps and Sacs of the Human Teeth.



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THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL.

1. JANUARY 1839.

PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*On the Origin and Developement of the Pulps and Sacs of the Human Teeth.* By JOHN GOODSIR JUNIOR, Surgeon, Anstruther, Fifeshire.

> "Il est peu de sujets en medecine sur lesquels on ait tant écrit que sur les dents; deux cent volumes contiendraient à peine tout ce qu'on en a imprimé! Mais est-ce à dire que tout soit connu à cet egard? Est-ce à dire que la matière ait été epuisée et qu'il ne reste plus rien à faire? Nullement. L'Anatomie n'a pas encore le dernier mot de la nature sur cet intéressant sujet, et il reste encore, quoiqu'on en dise, quelques doutes à eclaircir et plus d'une difficulté à résoudre."
>
> BLANDIN, Anat. du Systeme Dentaire, 1836.

SECTION I.—EXAMINATIONS OF THE DENTAL ARCHES AT DIFFERENT AGES.

1. AN embryo (Fig. 1,) which measured 7½ lines from the vertex to the point of the coccyx, weighed 15 grains, and appeared to be about the sixth week,[*] was selected and prepared for the purpose of examining the state of the palate and dental arches.

---

[*] It is difficult to determine the exact age of an embryo. The ages given in the text, therefore, must be considered as approximations, being probably rather underrated. I have given a full-sized sketch of the youngest subject in which I have observed any of the phenomena of dentition, with the weight and measurements of a few of the others. In researches of this kind, the sequences of phenomena are of more importance than their periods of appearance.

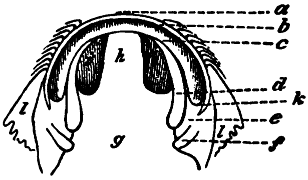
Velpeau, Embryologie ou Ovologie Humaine—Breschet, Etudes Anatomiques, &c. &c. de l'œuf dans l'espece humaine—Sœmmering, Icones Embryonum Humanorum.

Fig. 1.

The cheeks were divided transversely from the commissures of the lips with fine scissors; the jaws were separated, removed, and fixed to the bottom of a small capsule full of water. The point of the tongue was removed. The configuration of the mouth was then determined by means of a half-inch lens and two needles, bent at the points, and fixed in slender handles.

Upper Jaw.—The roof of the mouth was bounded anteriorly and laterally by the free edge of the lip, (*a*, Fig. 2,) which is at this age thin and of great transverse extent. Within the lip (*a*,) but separated from it by a groove, (*b*,) to be more particularly described afterwards, there was observed a lobe of a horse-shoe shape (*c*,) narrow anteriorly at the median line, broader, flatter, and of a rounded form on each side posteriorly. Coming out from above the internal posterior edges of this lobe (*c*,) and firmly adhering to it, two other lobes (*d d*) were seen; flat, rounded, and curving backwards and inwards posteriorly, gradually disappearing by pointed extremities anteriorly. From the posterior extremities of each of the lobes now described (*d d*,) and of the horse-shoe lobe (*c*,) a thin semitransparent membranous fold (*e e*) passed backward on each side, attached externally to the sides of the capacious bucco-pharyngeal cavity, bounded internally by a free edge opposed to its fellow of the opposite side, and terminating posteriorly on the lateral walls of the pharynx. Adhering to the inferior surface of each of these folds was seen a smaller lobe (*f f*) somewhat similar to the two last, and situated a little behind them. The needle placed under the folds showed that they were free and floating, except at their exterior or adherent edges, and that they constituted a partial division of the large common nasal, buccal and pharyngeal cavity into a superior and an inferior compartment. The upper wall of this common cavity was smooth and flat posteriorly (*g*); but anteriorly it was contracted and terminated in a longitudinal bar (*h*,) which ran forwards to be attached to the superior surface of the horse-shoe lobe at the median line, and to the other parts in that neighbourhood. Under the bar (*h*) a deep cavity (*i i*) was seen, which communicated with the exterior of the face by two small *foramina*, which constituted at this period the whole external nasal organ. As before-mentioned, a groove (*b*) was observed between the lip (*a*) and the external edge of the horse-shoe lobe (*c*.) This groove (*b*) was deep, and its walls and lips were in close apposition. It terminated posteriorly on each side (*k k*) by

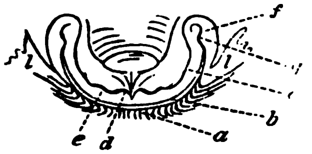
Fig. 2.



becoming more shallow, and curving backwards and inwards on the inferior surface of the membranous folds (*e e.*) There was a median frenum between the lip and the horse-shoe lobe.

Lower Jaw.—The under lip (*a*, Fig. 3,) resembled the upper, and was separated along its whole extent by a groove (*b*) similar to the one above, from a semicircular lobe, (*c.*) Anteriorly this lobe (*c*) was divided into two median large (*d d*,) and two lateral smaller lobules (*e e*,) the whole being firmly adherent to the floor of the mouth in front of the tongue and its *frenum*, which were both well developed. The lateral parts of the lobe (*c*) were rather indistinct, but at the point where the free edge of the lip terminated, it extended transversely and posteriorly, became thick and bulbous (*f f*,) and exhibited on its surface a narrow shallow groove of a sigmoidal form (*g g*,) which was continuous with the groove behind the lip. There was a median labial frenum.

Fig. 3.

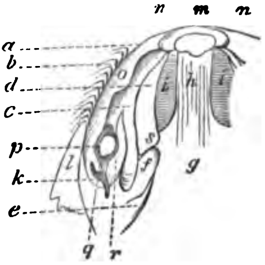


On the external sides of the membranous folds in the upper, and of the posterior parts of the lobe in the lower jaw, the cut surfaces of the cheeks made by the scissors were seen, (*l*, *l*, *l*, *l.*)

The mucous membrane over its whole extent was thin, and of a grayish-yellow colour, the lobes granular, very friable, and of a dead white. The breadth of the upper alveolar arch was 1$\frac{1}{3}$ line, the length of the same was 1 line.

2. The jaws of an embryo which measured 1 inch, weighed 20 grains, and appeared to be about the seventh week, were prepared and examined as in the former case.

Fig. 4.



Upper Jaw.—The free edge of the lip (*a*, Fig. 4,) was not so extended as at the sixth week. The horse-shoe lobe (*c*,) had become broader and more developed posteriorly, and anteriorly exhibited three lobules, one median (*m*,) and two lateral and anterior (*n n.*) The two lobes observed on each side of the palate in the former embryo (*d d*, *f f*, Fig. 2,) had disappeared, having apparently coalesced; the posterior one (*f*) being curved forwards to join the anterior (*d*,) in the point (*s*, Fig. 4,) while the combined mass had contracted itself towards the front of the mouth within the limits of the horse-shoe lobe (*c*.)

The cleft had slightly diminished, but was still of sufficient width to display the whole of the undivided nasal cavity.

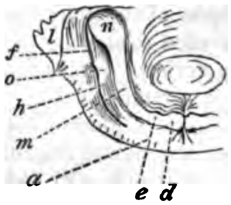
The lip (*a*) was so lax as to admit of being moved by the middle. The horse-shoe lobe (*c*) could also be pressed by the same means inwards and backwards. When these two parts were separated, the mucous membrane was seen to form a duplicature (*b*,) between the lips and a ridge (*o*,) which extended from the posterior part of the dental arch, to the outer extremity of the lateral lobule (*n*.)

The median portion of the dental arch was formed by the two lateral lobules (*n n*,) which separated the lips from the median lobule (*m*,) and extended also a little on each side of it.

The lateral portions of the arch presented externally the ridge (*o*,) formerly mentioned, smooth and convex on its external surface, internally moulded into three curves, the anterior long and shallow, the second deeper, the third or posterior almost semicircular. Behind the last curve, the internal edge of the ridge formed a deep notch, which swept outward and forward, so as to mould the former into an almost isolated lobule (*q*.) The ridge now disappeared, but its edge continued backwards and inwards, winding around the posterior extremity of the horse-shoe lobe (*c*,) so as to form a groove (*k k*, Figs. 2 and 4,) on the surface of the soft mucous membrane. The internal division of the lateral parts of the dental arch was formed by three bulgings, apparently productions from the horse-shoe lobe (*c*,) and which were separated from the curves of the ridge (*o*,) by a groove which was deeper at their sides than in their intervals. The anterior one was lengthened and indistinct, the middle one was more developed, the posterior circular, convex, and altogether isolated. The isolation of this bulging was produced by a longitudinal lobule (*r*,) apparently cut off from the external edge of the horse-shoe lobe (*c*,) and forming a partial inner ridge corresponding with the outer one. This new lobule (*r*,) reached back as far as the posterior extremity of the horse-shoe lobe (*c*,) and terminated anteriorly near the middle of the centre bulging.

Lower Jaw.—In the situation of the dental arch, there existed a groove (*h*, Fig. 5,) very distinct posteriorly, but having no outer lip anteriorly. The inner lip (*m*,) presented posteriorly a large lobe (*n*,) under which the needle was easily inserted for a short distance. In the middle, this lip (*m*,) was thin, elevated, and curved over the groove (*h*.) Anteriorly it became broader, and

Fig. 5.

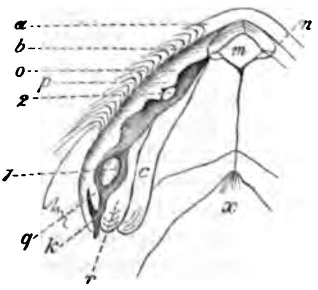


curved still more over the groove, and was divided into two median larger lobules (*d*,) and two lateral smaller (*e*.) Between the two median (*d*,) there was a notch at the attachment of the lingual frenum. The outer lip (*f*,) was deficient anteriorly, so that the groove was bounded in that situation by the under lip (*a*,) which was loose, free, and turned outwards. Posteriorly the outer lip (*f*,) was well developed, and came out from under the posterior lobe (*n*,) of the inner lip, so as to render the groove (*h*) pointed, and curved backwards and inwards. This lip (*f*,) extended only about half way towards the median line, and appeared flat, or in the same continuous plane with the floor of the groove. It was also curved outwards, so as to overhang the labial mucous membrane.

The groove presented an elevation (*o*,) of its floor, near its posterior extremity. There was a labial frenum. The mucous membrane possessed the same physical properties as at the sixth week. The lobes were not so granular, but tougher and more consistent. Breadth of superior arch 1½ line, length 1.

3. The jaws of an embyro at the second month having been prepared in the usual manner, presented the following appearances.

Fig. 6.



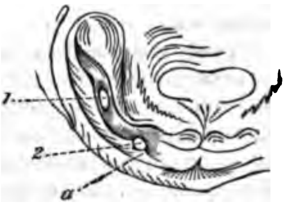
Upper Jaw.—The lip (*a*, Fig. 6,) was more movable, and its free edge less extended. The cleft in the palate had diminished, existing only as a small angular deficiency (*x*,) in the pendulous portion. The horse-shoe lobe was still perceptible under the form of a bulging (*c*,) represented as turned aside to exhibit the objects under it. The lobule (*r*) had increased in size, so as to extend farther backwards, and to appear on the posterior lateral parts of the palate. The median lobule (*m*) had become triangular, the anterior edge being formed by the curve of the palate somewhat pointed in front, the lateral edges being straight and meeting in an angle behind, from which the median line of suture or raphe of the palate proceeded. The median lobule (*m*) had increased relatively, the lateral lobules (*n n*) only absolutely. The posterior portion of the dental groove (*k*) was longer, wider, and not so much curved.

The bulging or papilla (1) was more distinctly isolated; and at the anterior extremity of the second curve in the ridge (*o*,) another papilla (2) had appeared as a production from the latter. This papilla (2) was bounded externally by a lamina (*p*,) which was also a production from the edge of the ridge (*o*,) and was notched at its inner margin, where it was applied to the side of the papilla.

The dental groove then terminated in a point, at the outer extremity of the lateral lobule (*n*.) There was a labial frenum.

Lower Jaw.—The posterior portion of the dental groove had undergone no material change, but had become deeper, and contained in the situation of the elevation marked (*o*, Fig. 5,) a distinct rounded papilla (1, Fig. 7). Further on, another papilla (2) bounded externally by a notched lamina (*a*) had appeared. This combined papilla and lamina was exactly similar in its configuration and relations to that marked (2, Fig. 6). The anterior part of the groove had become more distinct, not because it had acquired an outer lip, but because its floor had risen above the level of the labial mucous membrane. There was a labial frenum.—The breadth of the superior arch was 1⅔ line ; length 1⅓.

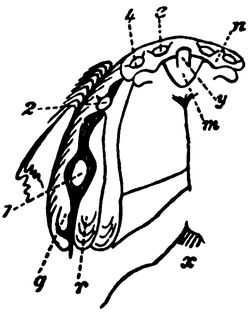
Fig. 7.



4. The jaws of an embryo nine weeks old were examined under water.

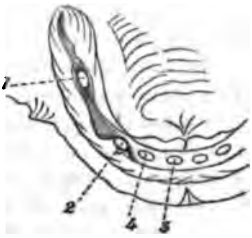
Upper Jaw.—No material change had taken place in the configuration of the palate, except that the median lobule (*m*, Fig. 8,) had diminished relatively, and in the transverse direction, while the lateral lobules (*n*, *n*,) had increased relatively, and also in the transverse direction. A longitudinal lobule (*y*,) had also appeared on the surface of the median lobule (*m*.) The cleft (*x*) in the soft palate was smaller. The posterior part of the dental groove was wider. The papilla (1,) had become more prominent, and the lips of the groove had almost met before and behind it. The papilla (2) is larger. A little further on, corresponding with the lateral lobule (*n*.) on each side, two papillæ (3 and 4,) with notched laminæ in front of them, had appeared. The centrals (3,) or those on each side of the median line, were the most distinct.

Fig. 8.



Lower Jaw.—The lips of the dental groove had approached so as to require separation by the needle to exhibit its contents distinctly. The papilla (1 or 2, Fig. 9,) had undergone little change, but two very indistinct bulgings (3 and 4,) had appeared on each side of the labial frenum, the centrals being the largest.—The breadth of the superior arch was 1⅝ line; the length 1⅓ line.

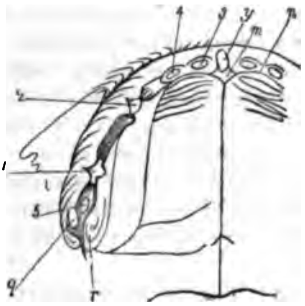
Fig. 9.



5.—In an embryo of the tenth week the following appearances presented themselves.

Upper Jaw.—Very little change had taken place in the lateral lobules (*n n*, Fig. 10,) or the median (*m*,) and its additional lobule (*y*). They had all increased absolutely, and if any relative change had taken place, it was in the transverse diminution of the median (*m*,) and the movement forward of its additional lobule (*y*). The palate had advanced anteriorly, so as not only to have encroached in some degree upon the median and lateral lobule, but also to have thrown itself into folds immediately behind them. The outline of the horse-shoe lobe (which is represented in the sketch as turned aside to exhibit the dental groove) was still observed. There was an indistinct uvula. The papilla (1 and 2) had sunk completely into follicles, and could only be seen by looking into the open mouths of the latter. The mouth of (1) was bordered by four laminæ or lids, that of (2,) by three, as represented in the sketch. The papillæ (3 and 4) had not increased much, but their notched laminæ had become more distinct. At the posterior extremity of the floor of the dental groove on the inner side of the lobule (*q*, Fig. 4, 6, 8, 10,) a slight bulging (5, Fig. 10,) was seen.

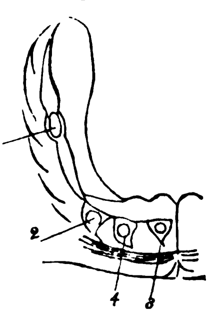
Fig. 10.



The upper lip had receded in the neighbourhood of the median line, so as to have disappeared almost entirely at that spot, the centre of the upper dental arch being exposed.

Lower Jaw.—The bulgings on each side of the median line (3, 4, Fig. 11,) which were so indistinct in the last subject, had become well developed, and inclosed in follicles, through the mouths of which they were seen. A similar change

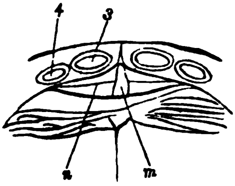
Fig. 11.



was observed in reference to the papilla (2). The follicles had been produced, by the stretching across of productions from the outer lip, (which was very indistinct,) towards similar but much smaller productions from the inner lip, (which was still very prominent.) The lines of junction of the septa were visible, and the mouths of the follicles presented an unfinished appearance. The papilla (1) had become surrounded by an incomplete follicle, in consequence of the production of a notched lamina from the outer lip of the groove, which lamina was almost met by a smaller slip of membrane from the inner lip.—The breadth of the superior arch was 2 lines, length 1⅔ line.

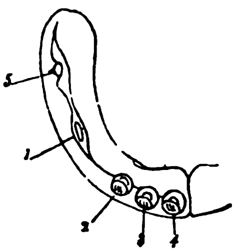
6.—11*th* or 12*th week.—Upper Jaw.*—The median lobule (*m*, Fig. 12,) had diminished so much transversely, as to have become antero-posterior; while its supplementary lobule had become attached to the frenum of the lip. The lateral lobules (*u u*) had increased much transversely, and appeared each to be divided into an anterior and a posterior portion. They were compressed by the true palate, which was folded at this part as at the tenth week into wrinkles, the longest and anterior of which, stretched across the median line from the right to the left side. The papillæ (3 and 4) with their follicles were fully developed. The other two papillæ (1 and 2, Fig. 10,) had not undergone much change, but the small bulging (5, Fig. 10,) had now become a distinct papilla, and its follicle had begun to show itself. The uvula was well marked.

Fig. 12.



Lower Jaw—The lines of junction of the interfollicular septa had almost disappeared, and the mouths of the follicles had become more distinct. The mouths of the three anterior follicles had an anterior lip, the free edge of which was directed somewhat inwards. It was necessary to lift up this lip with the needle to obtain a view of the contained papilla. At the posterior part of the dental groove, another papilla with a notched lamina, both productions from the external lip, had appeared (5, Fig. 13).—Breadth of superior arch 12½ lines; length, 2.

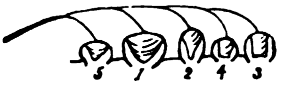
Fig. 13.



7.—13*th week.—Upper Jaw.*—There was little change in the configuration of the palate since the former week. The lobe running across the median line was still visible. The frenum of the upper lip had become closely attached to, and continuous with, the median lobule. The outlines of the horse-shoe lobe were still perceptible, and on its external side the lobule, all along marked (r,) was visible. The outer lip of the dental groove, or the external alveolar process, was equally developed all around. The upper lip was still much retracted. There were ten papillæ, inclosed in open-mouthed follicles, and ranged at nearly equal distances all around the dental groove. * The four anterior papillæ were flattened from before backwards with a straight edge, and were somewhat similar to the future incisive teeth. The next one on each side was a simple cone. The two posterior on each side were also conical, but flattened transversely, so as considerably to resemble carnivorous molars. Each of these papillæ adhered by its base to the fundus, while its apex, as during the eleventh and twelfth weeks, presented itself at, or, as in the present instance, protruded from, the mouth of its follicle. The point of the needle could be introduced through the mouth, so as to move the papilla about in the interior of the follicle.

By removing the outer lip of the dental groove, and the outer wall of all the follicles by the scissors, a good view was obtained of the configuration of these parts (Fig. 14). The follicles were observed to be mere duplications of the membrane of the groove, and consequently of the *general gastro-intestinal mucous membrane*. The inner surface of the follicles was of a greyish-yellow colour. The papillæ had increased relatively so as to protrude from the mouths of their follicles. They were granular, friable, and of a dead-white colour.

Fig. 14.



Lower Jaw.—No remarkable change had taken place in the lower jaw, except in the relative enlargement of the papillæ, and in the distinct developement of the follicle of the posterior papilla (5, Fig. 13). The outer lip of the dental groove was not very distinctly marked, but the inner was well developed.—The breadth of the superior arch was 3 lines, and the length was also 3 lines.

8.—14*th Week.*—*Upper Jaw.*—The median lobule had undergone little change, the lateral lobules had become broader from before backwards, apparently in consequence of the retraction of the palate, which, instead of exhibiting on its anterior part the confused transverse wrinkles formerly mentioned, presented on its lateral divisions, (corresponding to the horse-shoe lobe,) four or five parallel rugæ, which were apparently remains of the wrinkles. The upper lip had again become full, so that its free edge was on a level with the surface of the palate. The soft outer edges of the palate and the anterior edges of the lateral lobules were now closely applied to the outer lip of the dental groove, so as to close the latter in a valvular manner. When these edges, viz. the continuous semicircular outline of the whole palate, were raised by the needle, the dental groove with its contents, viz. ten papillæ in their follicles, were seen. It was observed that the follicles had increased relatively, the papillæ only absolutely, in consequence of which the latter, instead of projecting from, had receded within, the mouths of the former. The mouths of the follicles had apparently become smaller. This had arisen in consequence of the greater developement of the laminæ which were seen in the earlier stages. There were two, an anterior and a posterior, for the four anterior follicles, three, an internal and two external for the third, on each side, and four for the two posterior on each side. (Fig. 15.) Close upon the inner side of the mouth of each of the follicles there was observed a little depression in the form of a crescent, its concave edge being towards the former. These depressions were most distinctly marked at the four or six anterior follicles, where they were situated immediately behind their inner lips, (*a a a a a.*)

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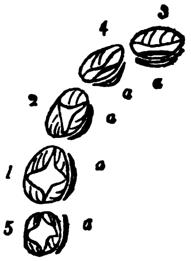
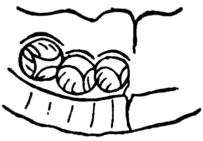
* Arnold, Salzburg Med. Chirurg. Zeitung 1831. Erster Band, p. 236. Valentin, Handbuch der Entwickelungs-geschichte des Menschen, p. 482.

Fig. 15:



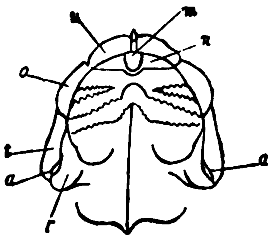
Lower Jaw.—The papilla had receded. The laminæ of the follicles were more developed. (Fig. 16). Little depressions or lunulæ had appeared similar to those in the upper jaw. When the membrane of the dental groove, with its adherent follicles and their pulps, was stripped off, the dental nerves and vessels were found running along under the follicles, and distributing vascular branches, and a nervous twig to each of them. (Fig. 14). Each of the individual follicles with its papilla, vascular branches, and nervous twig exactly resembled a large hair bulb with its nerve and vessels exposed after the hair has been extracted.—Breadth of the superior arch, 3½ lines; length, 3 lines.

Fig. 16.



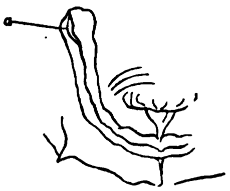
9.—15*th Week.*—*Upper Jaw.*—The outer edges of the palate, which in the last embryo lay unattached on the outer lip of the dental groove, in the present subject adhered firmly to it, except along a small portion posteriorly. (*a*, Fig. 17.) This adhesion was firm anteriorly on both sides of the median line, then became weaker, and posteriorly at the non-adherent portion (*a*,) between the lobules (*r* and *t*,) the lips of the groove retained their original smooth edges. When the lips of this non-adherent portion were separated by the needles, its floor and walls exhibited nothing but the greyish-yellow mucous membrane of the original groove. The outer lip of the dental groove was visible all around the external margin of the palate, and was divided on both sides into three parts, an anterior (*u*), a lateral (*o*), and a posterior (*t*). On the inner side of the latter was seen the longitudinal lobule, which has hitherto been marked (*r*). The median lobule (*m*) was rounded anteriorly, and had a process (*y*, Figs. 8 and 10,) which stretched forwards between the lobules (*a a*). This was the additional lobule formerly mentioned. The sides of the median lobule were straight and converged to its posterior extremity, which was circular, and was received into a curve, in the middle of a transverse band, constituting the anterior boundary of the palate, which appeared to have receded still more than in the last subject, and to have exposed still more completely the lateral lobules (*n n*). The four rugæ seen in the last subject had become ridges beautifully crenated, and converging, as represented in the sketch, towards a curve, reversed and opposite to the one formerly mentioned in the middle of the transverse band of the palate. This last curve was the result of the anterior junction of the lobes, (*d d*, Fig. 2,) and was traced through all its phases to its present state. The median suture of the palate proceeded from it posteriorly. The dental groove being torn open by means of the needles, its lips were found to have adhered pretty firmly, as before-mentioned, but a feeble adhesion only had taken place between its walls, so as to allow its contents to be restored to their original condition by means of a blunt instrument. This was carefully done under water, and the mouths of all the follicles with their laminæ were displayed. The latter were more developed than in the last subject, and completely concealed the papilla. The former required to be lifted up in order to display the latter. Careful observation during the separation of the contents of the groove disclosed the important fact, that the general adhesion had not obliterated the little crescent-shaped depressions behind the mouths of the follicles. These retained the smooth greyish-yellow colour of the walls of the original groove, and from this circumstance could be distinguished from the general flocculent appearance of the other parts.

Fig. 17.



Lower Jaw.—The outer lip of the dental groove had increased in size, and was as prominent as the inner, except posteriorly, where the latter still retained its posterior lobe; but the most remarkable change which had taken place since last week was the complete adhesion of both lips, as in the upper jaw, with the exception of a small portion posteriorly, which still retained the peculiar appearance of the dental groove, and in which nothing could be seen but the smooth mucous membrane; (*a*, Fig. 18.) When the dental groove was torn open, as was done in the upper jaw, the laminæ (which were highly developed) of the follicles, and the walls of the groove, were found to be rough and flocculent from adhesions, with the exception of the little depressions formerly mentioned, which still retained their original appearance.

Fig. 18.

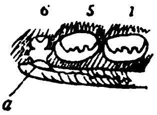


Breadth of the superior arch, 5 lines; length, 4 lines.

10.—16*th week.* — *Upper Jaw.*—The palate retained the appearance it had in the last subject, with the exception of the median lobule, which had become narrow in front, and broad posteriorly. The raphe of the dental groove had become firmer, so as to give a much more defined and permanent appearance to the non-adherent portion posteriorly, which was now seen to great advantage, its fine greyish mucous membrane gradually running at its edges into the white tough substance of the palate and gums.

Having separated the lips of the non-adherent portion (*a*, Fig. 19,) a papilla, sunk in an open follicle, with three or four laminæ, was visible, (6.) The membrane of the palate and maxillary arch being stripped from the bone, and its surface of adhesion examined, lines corresponding with the sutures of the bones were observed; one the median, another the intermaxillary, and a third with the palato-maxillary. Five tooth sacs were also observed on both sides of the maxillary arch. These were divided into three groups, two in the first, or anterior, one in the second, and two in the third, or posterior. These groups were covered with a flocculent spongy membrane, which was easily stripped off by the forceps, and when this was carefully done, it became evident that the sacs which were formerly grouped together by this membrane were individually isolated, and formed of a thin gray diaphanous membrane, similar to the one formerly mentioned as covering the bottom of the dental groove, and constituting the membrane of the follicles. The careful detachment of the external

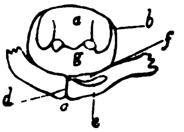
Fig. 19.



spongy membrane from the posterior group showed, what was not at first observed, that there was at the posterior part of the posterior sac another very small one, which by careful examination was seen to be the fundus of the open follicle in the non-adherent portion of the dental groove.

The adhesion of the lip and walls of the groove had now become so strong, that it was impossible to separate them. The only way, therefore, in which its contents could be examined was by transverse sections. When these sections were made between the different sacs, they displayed scarcely any traces of the dental groove; but when they passed through any place perpendicular to the surface of the gum, and near to the middle of any of the sacs, they exhibited the appearances represented in the marginal sketch, (Fig. 20.) The deciduous tooth pulp, (*a*,) which was lately a free papilla; (*b*,) the section of its sac, which was a follicle when the pulp was a papilla; (*d*,) the line of adhesion of part of the walls of the dental groove leading from the shut sac to (*c*,) the raphe of the groove; (*e*,) the section of the non-adherent portion of the groove in the situation of the lunula, which existed behind (*f*,) the inner laminæ of the sac (*b*,) in its former follicular condition. From the consideration of this section, (Fig. 20,) the mode in which the original follicle, the non-adherent depression behind the inner laminæ, and the walls of the dental groove, were connected after full adhesion of all the neighbouring parts, will be easily understood. The little cavity (*e*) adhered by its anterior and inferior extremity to the line of adhesion, (*d*,) so that it and the sac of the milk tooth were both connected to the raphe of the edges of the dental grooves by lines of attachment, which resembled two petioles proceeding from a common footstalk. These lines of attachment were not tubular, but resisted all efforts to push a fine probe or bristle through them; they were merely opaque remains of the surfaces of junction contrasting with the semitransparent substance of the gums. Parallel sections through all the sacs exhibited similar appearances. When the contents of the sacs were examined, the pulps were found to have acquired the configuration of the bodies of the future teeth. The bases by which the molar pulps formerly adhered to the bottoms of their sacs, and which may be denominated their primary bases, had become almost divided into three secondary bases, which corresponded with the internal and two external fangs of the future teeth. This division was so far accomplished by the advancement of the internal grey membrane of the sac, under the form of small compressed canals between the base of the pulp and the external spongy membrane. These canals, which were three in number, one external

Fig. 20.



and two internal, did not meet in the middle under the pulp. Deposition of ***tooth substance*** (Zahn-substanz) had commenced on the edges and tubercles.

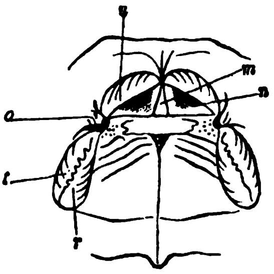
The sacs were twice as large as their contained pulps, and in the space (*g*, Fig. 20,) which existed between them, there was observed a very soft flocculent gelatinous substance, which had no attachment to the pulp, and did not appear to adhere to any part of the sacs, except the laminæ, and the parts adjoining them.

Lower Jaw.—The adhesion of the dental groove was not so strong as in the upper jaw. The open portion (*a*, Fig. 18,) was fully defined, and exhibited on its floor the orifice of a follicle, containing a papilla. In other respects the lower was similar to the upper-jaw.

Breadth of superior arch 7 lines; length 5 lines.

11.—*5th month.*—Fœtus minutely injected with size and vermilion.

Fig. 21.



Upper Jaw.—The lobes (*t*, *o*, *u*, Fig. 21,) had become highly developed. The anterior one (*u*) was convex anteriorly, with a sharp edge directed backwards, and corresponded with the incisive teeth. The central lobe (*o*) had become shorter, but more prominent, like a canine tooth. The posterior (*t*) had united firmly with the longitudinal lobe all along marked (*r*,) so as to close the open portion of the groove (*a*, Fig. 17,) which was described in the two last subjects. The raphe of the groove between these two lobes was serrated, and a vessel was seen traversing each denticulation. The raphe then ran close along the inner edges of the bases of the lobes, (*o* and *u*.) The median lobule was triangular, the base posterior; the apex in front continuous with the labial frenum, and situated between the anterior pointed extremities of the lobules, (*u*, *u*.) The lateral lobules were very distinct. The other less important changes which had taken place in the palate may be understood by comparing Figs. 21 and 17.

The membrane of the palate with the sacs of the teeth were removed from the bone. The fundus of the follicle (6, Fig. 19,) had now assumed the appearance of a sac, and the other ten, instead of being grouped, had become isolated. The branch of the dental artery, which supplied each of the sacs and their pulps, was seen when it reached the fundus of the former to give off a number of twigs, which, radiating from their common centre, proceeded perpendicularly towards the gum, near which they inosculated with others proceeding from it. The combined vessels then formed a pretty minute net-work in the spongy membrane formerly described.

Transverse sections were now made by the scissors through all the sacs. The general appearance of these sections was similar to that of those at the 4th month, but the gelatinous granular substance between the pulp and the sac was of the consistence of very firm jelly, closely and intimately adherent to the whole interior of the sac, with the exception of a narrow strip all around the base of the pulp, along which strip, the grey membrane of the sac retained its original appearance, and through which the radiating saccular twigs were visible, being strongly and beautifully contrasted with the cream-coloured surface of the granular substance. The mass of the granular substance had a peculiar greyish white colour, its surface was cream-coloured, and had a dry chalky appearance. It had a tendency to tear in a direction perpendicular to the internal surface of the sac. Although closely applied it did not adhere to the pulp, but, as stated above, surrounded it on all sides till within a short distance of its base—"whatever eminences or cavities the one *had*, the other *had* the same, but reversed, so that they *were* moulded exactly to each other." In the incisives its principal mass lay—"against the hollowed inside of the tooth, and in the molars, it *was* placed directly against their base like a tooth of the opposite jaw." In the pulps of the molars, which had three canals which *now* passed completely across their bases, the granular substance sent a process into each of them. These processes did not meet in the centre, but disappeared near to it, and left, as in the case of the general mass, a minute portion of the grey membrane of the sac, between themselves and the secondary bases of the pulp.[*] In the case of the molars also the dental arterial branch divided into three twigs, one for each secondary base of the pulp, and from all of these, radiating perpendicular ramuscules proceeded as in the case of a pulp with a primary base.

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[*] The only authors, as far as I know, who have observed and described this gelatinous body, are Mr Hunter in his Natural History of the Teeth, p. 94, and Purkinje and Raschkow, in the work of the latter, entitled, "Meletemata circa Mammalium dentium evolutionem"—Not having been able, hitherto, to procure Raschkow's work, I can only state from Burdach (Physiologie, French ed. Tom. iii. p. 498), that Purkinje's opinion appears to coincide with Mr Hunter's as to its being the organ which secretes that enamel. Mr Hunter has not described the processes which this body sends under the pulp, or the space left between it and the base of the latter, but his description is in other respects so correct and characteristic that it is difficult to account for the manner in which the first part of his chapter on the formation of the enamel has been so much misunderstood. Dr Blake (p. 34,) (although he described the granular body as the inner membrane of the tooth sacs, and as possessing "no vessels capable of conveying red blood) supposed that Mr Hunter meant by "another pulpy substance," the sacs of the permanent teeth. Mr Bell also in a note, Vol. ii. Palmer's ed. Hunter's works, p. 43, states that after most accurate observations, he had come to the conclusion that the "pulpy substance" mentioned by Hunter, is nothing more than the inner membrane of the sac turgid with blood and earthy matter preparatory to the secretion of the enamel.

The arterial net-work, which was formed in the external spongy membrane by the inosculation of these vessels with those proceeding from the gums, transmitted small branches which ramified with such minuteness in the substance, and on the surface of that portion of the gray membrane to which the granular matter adhered, that, when the latter was removed, the former appeared to the naked eye a mass of vermilion, but under a one-fourth of an inch lens exhibited a net-work of the most minute injection. No injected vessel could be seen in the granular substance.* The main dental twig after giving off all these branches, arrived at the base or secondary bases of the pulp, and immediately divided into many branches, which ramified in a contorted flattened position, between the base or bases of the pulp and the membrane of the sac. From these, smaller ramifications were transmitted into the substance of the pulp, which ramified in considerable numbers in the centre of its mass, but scarcely at all near its surface or on its membrane, except in the neighbourhood of, and at the point where, deposition of *tooth substance* had commenced, immediately beneath which the vascularity was intense, both in the substance under, and on the surface a little beyond, the edge of the scale.† This surrounding vascularity had the appearance of a zone, and was situated in the substance, and on the surface of an elevated portion of the pulp, which surrounded the scale of tooth substance.

The granular substance in contact with the tooth substance and its border had begun to be absorbed, and had consequently become thinner in that situation than elsewhere, allowing the subjacent vascularity to appear through it. No vessel could be detected in the granular substance to account for the absorption of its inner surface.

The ten little cavities had undergone no change, except that the two or four anterior had become rather longer, and were situated farther from the surface of the groove, so as to be placed rather behind than below the sacs. The anterior cavity, in particular, although its walls were still in contact, and required to be separated by the needles under water to see its interior, had become pear-shaped. The fundus or portion farthest from the gum exhibited on its floor a fold, which lay in the direction of the edge of the future permanent tooth, and near its apex there were two other minute folds, one on the anterior wall, the other on the posterior. Beyond this the cavity terminated in an opaque IMPERVIOUS line, which soon disappeared. The substance of the gums had become infiltrated with a quantity of gelatinous matter very similar to the granular substance of the sacs. In consequence of this infiltration the line of junction of the walls of the dental groove had become obliterated, the substances of the gums had become thicker, and the sacs more removed from the surface.

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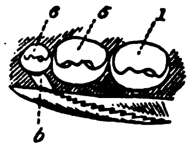
* Blake, Essay on the Structure and formation of the Teeth, p. 4.

† Serres, Essai sur l'Anatomie et la Physiologie des Dents, p. 20.

The open portion of the groove (*a*, Fig, 19,) had disappeared, but a longitudinal section showed that the *lips* only had adhered, the *walls* had not. The follicle (6, Fig. 22,) had become a sac, in consequence of which a cavity (*b*) remained between it and the surface of the gum. Gelatinous substance had been deposited in the sac (6), and in the neighbourhood of the cavity below it (*b*), as in the other sacs.

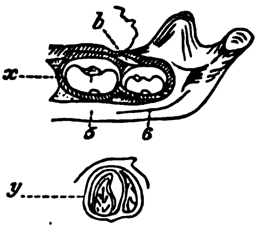
Fig. 22.



The lower jaw exhibited changes analogous to those in the upper.

12.—*Child at Birth*—A longitudinal section was made through the posterior part of the under jaw, when the sacs and pulps of the posterior milk molar, and of the first permanent molar, and the arrangements represented in Fig. 23 were observed. (5,) the sac and pulp of the posterior milk molar; (6,) the sac and pulp of the first permanent molar; (*b*) the cavity marked (*b*, Fig. 22).

Fig. 23.



The sac of the permanent tooth (6) was now almost wholly imbedded in the base of the coronoid process of the jaw. The cavity (*b*,) which was attached to the upper part of the sac of the permanent tooth by its posterior extremity, adhered by its anterior extremity, to that point of the gum which was attached to the anterior edge of the base of the coronoid process, so as to drag its surface at that point into a dimple. The cavity (*b*) was consequently longer than it was at its first formation.

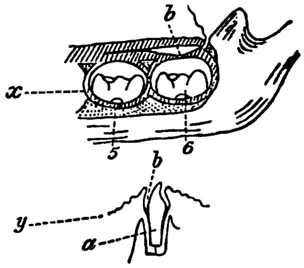
The granular substance had wholly disappeared. The interior of the sacs had a villous highly vascular appearance, like a portion of injected intestinal mucous membrane. The original opening of the sac (6) into the cavity (*b*) was indicated on its inner surface by an indistinct circular lip. The sacs of one of the central incisives of the same fœtus exhibited externally nothing peculiar. After a transverse section, it was found to be composed of two, he temporary and permanent combined.

The walls of the temporary sac (*b*, Fig. 23,) were composed of an external membrane, which was rather thick and condensed;

the inner could be separated from it, and had the appearance, as in the molar sacs, of an injected villous membrane. The little permanent sac was situated in the substance of the outer membrane of the temporary sac, as if the latter had been split to receive it. It was lined by a membrane similar to that of the temporary, and exhibited near the lower end of its posterior wall the incipient pulp, which was evidently a developement of the fold observed in that situation at the fifth month. It terminated towards the gum by an indistinct pointed extremity, from which a short opaque impervious line proceeded, near to which the anterior and posterior folds, observed at the five month, were seen.

13.—The lower jaw of an infant about eight or nine months old, in which the central incisives had cut the gum, was prepared by removing a section from its external posterior lateral aspect, so as to expose the sacs of the posterior milk molar, and of the anterior permanent molar (*x*, Fig. 24). The latter (6,) instead of being buried in the base of the coronoid process, was situated further forward and the cavity (*b*,) having been displayed by a longitudinal section of the former, was found, comparatively speaking, to have recovered its original small extent, being attached inferiorly to the top of the sac (6), and superiorly to the anterior edge of the base of the coronoid process.

Fig. 24.



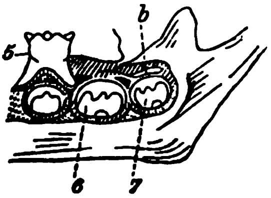
Upon examining the two incisive teeth which had cut the gum, it was found that a bristle could be inserted between their surfaces and the gum, for one-third of an inch. Through the soft parts a transverse section was made, which was afterwards continued through the jaw and one of the teeth by means of a very fine saw.

It was now observed that the tooth (*a*. Fig. 24 *y*,) had acquired nearly two-thirds of its fang, and that the *sac* had again become an open follicle (*b*). This follicle was shorter than the whole length of the tooth by the length of the protruding portion of the latter. At the mouth of the follicle, its lining membrane was continuous with the surface of the gums, and continued free till it arrived at the termination of the enamel, where it united to the surface of the fang of the tooth, but could be separated from it as a continuous membrane, and at the lower end of the root it became continuous with the surface of the pulp, whose base was yet considerable. Upon removing the bone in front of the neighbouring lateral tooth, which had not yet passed through the gum, it was observed that the extremity of its fang, or rather the fundus of its sac, was deeper in the jaw than that of the central, by a length equal to the protruding portion of the latter. This change of level had not, however, taken place in the case of the alveoli, that of the central being rather deeper than the lateral. The space intervening between the bottom of the alveolus of the central tooth, and the fundus of its sac, was occupied by a spongy filamentous tissue, through which the dental vessels and nerves proceeded.

14.—The lower jaw of an infant, which had cut all its milk teeth, and which was probably between four and five years old, was prepared in the same manner as the last.

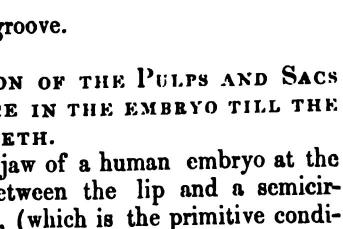
The sac of the anterior permanent molar (6, Fig. 25,) was situated under the gum in front of the coronoid process, and a new sac and pulp of a smaller size (7,) had appeared buried in the base of that process. The cavity (*b*) was again lengthened out, being attached anteriorly, at the anterior edge of the base of the process, to the gum, and posteriorly to the top of the new sac (7.) That portion of the cavity formerly attached to the sac (6,) was now almost obliterated.

Fig. 25.



15.—The posterior part of the lower jaw of a child about six years old was prepared by removing a section from its internal posterior aspect, and making at the same time a longitudinal section of the gum. The sac (7, Fig. 26,) had advanced from under the coronoid process; and another very small sac and pulp had appeared inclosed in a bony crypt under the process, and communicating through the upper part of the bony cell of the sac (7,) with the gum, where it terminated in an opaque line or tail, the last remains of the surface of adhesion of the dental groove.

Fig 26.



Section II.—A Description of the Pulps and Sacs from their First Appearance in the Embryo till the Eruption of the Wisdom Teeth.

When we examine the upper jaw of a human embryo at the sixth week, there is perceived between the lip and a semicircular lobe of a horse-shoe form, (which is the primitive condition of the palate) a deep narrow groove which terminates on each side, behind the former, by curving inwards on the soft mucous membrane. As this groove becomes gradually wider, and the lip more lax in a direction from behind forwards, there appears on its floor posteriorly, and proceeding in the same direction, a ridge (the external alveolar process,) which speedily divides the original groove into two others; the outer one, forming the duplicature of mucous membrane from the inside of the lip to the outside of the alveolar process, the inner one, constituting what may be very properly denominated the *primitive dental groove*, as the germs of the teeth appear in it.

The inner side of the ridge already mentioned, after being cut into three grooves whose concavities look inwards, and of which the posterior is the deepest, terminates in a rounded lobule, which is continuous with it anteriorly, while externally, internally, and posteriorly, it is bounded by that portion of the original groove which was situated behind the semicircular lobe. The curves of the ridge are occupied by bulgings of the semicircular lobe, so that the ridge and lobe with their curves and bulgings, are exactly similar to the arrangement of the mucous membrane of the second compartment of the stomach of the porpoise.

At some period between the sixth and seventh week a longitudinal portion is cut off, from the internal posterior edge of the semicircular lobe, extending as far forwards as the middle bulging, and about the same time the posterior bulging becomes isolated, and defined, under the appearance of an ovoidal papilla, the long diameter of which is antero-posterior. This papilla is the germ of the anterior superior milk molar tooth, the first tooth germ which appears in the developement of the human body. It is at this period a simple free granular papilla, like many others on the surface of the mucous membrane and skin.

About the eighth week or second month, a second papilla appears at the point of projection of the ridge, between the middle and anterior curve. This papilla, which is the germ of the superior milk canine tooth, is rounded and granular, and is bounded externally by a triangular lamina, which spreads out into, and is continuous with, the inner edge of the ridge, having its apex notched so as to fit the external aspect of the papilla.

During the ninth week the ridge advances in an indistinct manner to the median line, and there appears on each side of that line an oblong papilla with a notched lamina in front of it, and immediately afterwards another smaller papilla and lamina external to the former. These last papillæ are the germs of the incisive teeth, and are placed in connection with the lateral elements of the inter-maxillary system.

The primitive dental groove, which before the appearance of the incisive germs terminated anteriorly at the outer extremity of the lateral inter-maxillary lobules, now extends forwards to the median line. The longitudinal lobule, and the lobule opposite to it also, have lengthened out posteriorly, and the intervening portion of the primitive groove has become wider and not so curved. The sides of the groove before and behind the anterior molar papilla have been gradully approaching one another.

During {the tenth week, the incisive papillæ make very little advance, their anterior laminæ only increasing somewhat in size. Processes from the sides of the primitive dental groove, particularly the external one, approach and finally meet before and behind the papilla of the anterior molar, so as to inclose it in a follicle through the mouth of which it may be seen. A similar follicle is gradually formed round the canine by the advancement inwards of its external notched lamina, which at first appeared as a production of the ridge or external lip of the groove. The germ of the posterior milk molar also appears as a small papilla towards the end of this week behind the anterior molar, at the side and apparently as a production from the rounded lobule, which terminates posteriorly the outer ridge.

During the eleventh and twefth week, the incisives advance steadily. Septa pass between them from the outer to the inner side of the groove, so that their papillæ become completely sunk in well developed follicles. No material change takes place in the anterior molar or canine, but the posterior molar papilla enlarges, and the terminal lobule of the outer ridge folds gradually round it, so as to constitute its follicle, behind which there still remains a portion of the primitive groove.

The changes which ensue during the thirteenth week consist in the completion of the follicle of the posterior molar, and in the gradual change which takes place in the shape of the different papillæ. Instead of remaining, as hitherto, simple, rounded, blunt masses of granular matter, each of them assumes a particular shape. The incisives acquire in some degree the appearance of the future teeth; the canines become simple cones; and the molars become cones flattened transversely, somewhat similar to carnivorous molars. During this period, too, the papillæ grow faster than the follicles, so that the former protrude from the mouth of the latter, while the depth of the latter varies directly as the length of the fangs of their future corresponding teeth, the canine follicle being deepest, &c. &c. While the papillæ are changing their shape, the mouths of the follicles are undergoing a change which consists in the developement of their edges, so as to form opercula, which correspond in some measure with the shape of the crowns of the future teeth. There are two of these opercula in the incisive follicles, one larger, anterior, and rather external, the second smaller, posterior, and internal. There are three for the canines, an external and two internal, and four or five for the molars, each corresponding with a tubercle; while their edges correspond with the grooves on the grinding surfaces of these teeth.*

The inner lip of the dental groove (or the outer edge of the palate) which has been increasing for some time past, is now at the fourteenth week, so large as to meet and to apply itself in a valvular manner to the outer lip or ridge, which has also been increasing, The follicles at this time grow faster than the papillæ, so that the latter recede into the former. The molar papillæ gradually acquire two or three additional small compressed tubercles on their sides, and their apices become less conical, so that they still more resemble the molar teeth of the carnivorous mammals.† The opercula of the follicles continue to increase, so as almost to hide their contained papillæ.

The *primitive dental groove*, which at this period contains ten papillæ in as many follicles, and is situated on a higher level than at first, may be now more properly denominated the *secondary dental groove*. It is when in its secondary condition that the groove affords a provision for the production of all the permanent teeth, with the exception of the first or anterior molars. It is about the fourteenth or fifteenth week that we begin to observe preparations made for this provision, by the gradual appearance of a little depression in the form of a crescent, immediately behind the inner opercula of each of the milk tooth follicles. The concave edges of these depressions are in contact with the attached margins of these opercula. Those of the centre incisives appear first, then the laterals, canines, anterior bicuspids, posterior bicuspids. About this time the opercula close the mouths of the follicles, but without adhering, then the anterior closing first, the laterals, and so on in succession. The lips and walls of the secondary groove now begin to cohere in a direction from behind forwards, the opercula and every part of the groove, with the exception of the ten depressions for the permanent teeth, becoming rough, flocculent, and adherent. The follicles have now become the *sacs;* the papillæ the *pulps* of the milk teeth; and the crescent-formed depressions vacant *cavities of reserve*, to furnish delicate mucous membrane for the future formation of the *pulps* and *sacs* of the *ten* anterior permanent teeth.

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* It would be interesting to ascertain whether the opercula of the human tooth follicles may not be rudimentary organs, which are to attain their utmost developement in the sacs of the elephantoid, ruminant, and other compound teeth, under the form of depending folds for the secretion of the intersecting enamel and cement plates.

One may easily conceive the mode of formation of a composite tooth sac, by supposing the opercula, after their edges have met, to dip down back to back between the divisions of the pulp, till they almost meet the common body of the latter.

† This is another instance of the law of progressive developement, by virtue of which an organ, in the course of its formation, passes through phases, which correspond to permanent conditions of the same organ in other animals. A human molar tooth pulp is at first rounded, as in certain fishes; then conical, as in other fishes and reptiles; then conical, but flattened transversely, gradually acquiring two or more additional conical tubercles, as in the carnivorous mammals; and finally, by the equalization of the primary and secondary tubercles, assuming the shape of the molars in the quadrumanous animals and man. In the elephantoid, ruminant, and rodent animals, it probably undergoes a further and ultimate change in the deepening of the rudimentary grooves on the grinding surface.

The general adhesion does not invade that portion of the primitive dental groove which is situated behind the posterior milk molar follicle. This small portion retains its original appearance, greyish-yellow colour, and smooth edges for a fortnight or three weeks longer, and affords a nidus for the developement of the papilla and follicle of the anterior permanent molar tooth, the fundus of its follicle being situated immediately behind the sac of the posterior milk molar. The *cavities of reserve* for the ten anterior permanent teeth are at this period minute compressed sacs, with their sides in contact, and situated between the surface of the gum and the milk sacs.

The papillæ of the milk teeth, from the time that their follicles close,* become gradually moulded into their peculiarly human shape. The molar pulps begin to be perforated also by three canals which, proceeding from the surface to their centres, gradually divide their primary base into three secondary bases, which become developed into the fangs of the future teeth. While this is going on, the sacs grow more rapidly than the pulps, so that there speedily exists an intervening space, in which is deposited a gelatinous granular substance, at first in small quantity, and adherent only to the proximal surfaces of the sacs, but ultimately about the fifth month, closely and intimately attached to the whole interior of these organs, except for a small space of equal breadth, all round the base of the pulp, which space retains the original grey colour of the inner membrane of the follicle, and as the primary base of the pulp becomes perforated by the canals formerly mentioned, the granular matter sends processes into them, which, adhering to the sac, reserve the narrow space described above between themselves and the secondary bases. These processes of granular matter do not meet across the canals, but disappear near their point of junction. The granular matter is closely applied, but does not adhere to the surface of the pulp. "Whatever eminences or cavities the one has, the other has the same but reversed, so that they are moulded exactly to each other."

Each branch of the dental artery, as it arrives at the fundus of its destined sac, sends off a number of radiating twigs, which run in the substance of the cellular submucous tissue (which constitutes the outer membrane of the sac) towards the gum, from which others proceed to inosculate with them. The combined twigs then ramify minutely in the true membrane of the sac, without sending the smallest twig into the granular substance.[*] The dental branch, after giving off these saccular twigs, divides into a number of contorted ramifications between the base of the pulp and the sac, which from smaller ramusculi are transmitted into the pulp itself. In the case of the molars, the main branches divide into three secondary branches, one for each of the secondary bases. From these, three sets of saccular twigs, and three packets of contorted pulp vessels, take their origin.

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* Herissant in the "Mem. de l'Acadamie Royale," 1754, p. 664, described two gums, the "gencive permanent" and the "gencive passagere." His ideas on the subject appear to have been derived from the examination of jaws in which the lips and walls of the secondary dental groove "gencive passagere," had not become completely adherent or obliterated. In this way the indistinct mouths of the milk tooth sacs on the floor of the groove "gencive permanent," did not escape the notice of this most accurate observer. The cartilages of the gum described by Serres, Essai, p. 10, are to be considered as the walls of the groove in the semicartilaginous condition which they assume after closure.

While these changes have been taking place in the sacs of the milk teeth, the follicle of the first permanent molar closes, and granular matter is deposited in its sac. The walls of that portion of the secondary groove below it do not adhere; the edges alone do so. There is, therefore, a cavity of considerable size below the sac of this tooth, or between it and the surface of the gum. This cavity is a reserve of delicate mucous membrane to afford materials for the formation of the second permanent molar, and of the third permanent molar or wisdom tooth.

A little before this period, *tooth substance* begins to be deposited on the tubercles and apices of the pulps, which have acquired round the point of deposition a raised border and a zone-like vascularity; and synchronous with this deposition, absorption takes place on the inner surface of the granular matter immediately in contact with it. No vessel can be detected running to the point of absorption, but ultimately the granular matter becomes so thin as to allow the subjacent vascularity to appear. The absorption goes on increasing as the tooth substance is deposited, and when the latter reaches the base of the pulp the former disappears, and the interior of the dental sac assumes the villous vascular appearance of a mucous membrane. This change is nearly completed about the seventh or eighth month.

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[*] Mr Fox (Natural History of the Human Teeth, p. 20,) and Mr Bell (Anatomy of the Teeth, p. 54, and in a note, p. 39, Vol. ii. Palmer's edition of Hunter's Works,) have both misunderstood the statements of Mr Hunter and Dr Blake on the relative vascularity of the membranes of the tooth sacs. (Hunter's Natural History, p. 84, and Blake, p. 4.) What Blake denominates the internal lamella, is the enamel pulp of Hunter, Purkinje, and Raschkow, the gelatinous granular substance described in the text. He with great accuracy states, that it is "more tender and delicate, and seems to contain no vessels capable of conveying red blood." Under the denomination "external lamella" he includes the proper vascular mucous membrane of the sac, and the external spongy submucous tissue. In his search after the germs of the permanent teeth, Blake's attention appears to have been directed to the tooth sacs when in the condition he describes. Mr Hunter, again, who had a most correct conception of the constitution of the sacs, has, with his usual sagacity, not confounded the granular body, or, as he denominates it, "another pulpy substance," with the proper membranes of the sacs. Accordingly, in his account of the relative vascularity of the membranes of the sacs, he, when describing the manner in which a tooth is formed, has taken no notice of the pulpy susbtance. Dr Blake describes the membranes of the sacs at an early period; Mr Hunter again, in a child at birth, at which time the external membrane is not very vascular, and has assumed somewhat of the appearance of a fibro-cartilage.

Up to this period little change has taken place in the ten anterior, or in the two posterior or great cavities of reserve. The ten anterior have been gradually receding from the surface of the gum, so as to be posterior, instead of inferior, to the milk sacs. The two or four anterior began about the fifth month to dilate at their distal extremities, across which a fold appears (which is the germ of the future pulp) lying in the direction of the cutting edge of the future tooth ; and at the proximal or acute extremities of the cavities two other folds, an anterior and a posterior appear. [*] These round off the undefined apices of the cavities, and are strictly analogous to the opercula of the milk follicles.

The distal fold gradually acquires the appearance of a tooth pulp, while the proximal disappear by the obliteration of the little undefined space beyond them.

The cavities of reserve have now become tooth sacs, and under this form they continue to recede from the surface of the gum, imbedding themselves in the submucous cellular tissue, which has all along constituted the external layer of the milk sacs, and in which the larger saccular vessels ramify before arriving at the true mucous membranes of the sacs. This implantation of the permanent, in the walls of the temporary tooth sacs, gives the former the appearance of being produced by a GEMMIPAROUS process from the latter.[†]

The dental groove was originally imbedded in an alveolar groove. As the dental interfollicular septa are developed in the former, osseous septa also begin to be formed in the latter. These osseous septa are at first in the form of bridges, but ultimately, at the sixth month, become complete partitions. As the sacs increase in size, the alveoli increase also, and when the permanent form slight projections behind the temporary tooth sacs, niches [*] are formed for them in the posterior walls of the alveoli. Whilst this increase in the bulk of the sacs and alveoli is going on, there is no proportionate increase in the length of the jaw, in consequence of which the sac of the anterior permanent molar has been insinuating itself into, and at the eighth month, or the full time, is almost wholly imbedded in, the maxillary tuberosity, [†] and has become situated on a higher level than the milk sacs, during which it has not only drawn the surface of the gum upwards and backwards, but has also lengthened out the great or posterior cavity of reserve.

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[*] These two folds are strictly analogous to the opercula of the milk tooth sacs. They never attain, however, the same high developement as those of the latter, remaining in a rudimentary state, apparently in consequence of the almost saccular condition of the cavities of reserve. The existence of these laminæ in a rudimentary state proves, that, in the formation of the permanent teeth, there is a strict adherence to the law of follicular developement, even when, in man at least, there is no apparent necessity for it.

[†] It was this imbedding of the permanent, in the walls of the temporary, tooth sacs which deceived Dr Blake, and led him to suppose that the former derived their origin from the latter. Mr Fox supported the same view of the subject ; and Mr Bell, in his own work, (Anatomy, &c. &c. of the Teeth, p. 61,) and more lately in his notes in Palmer's edition of Mr Hunter's Works, Vol. ii. p. 37, has strongly urged the same doctrine. Mr Bell has stated, that Mr Hunter's "account of the manner in which the permanent teeth are formed is exceedingly imperfect ;" but it is evident, that, if the account of the origin of these teeth given in the text be correct, Mr Hunter was not in error when he supposed both sets to be of independent origin. Mr Hunter was so correct a thinker, that he did not account the circumstance of contiguity to be a proof of dependence. He was apparently ignorant of the origin of both sets, and in his usual cautious manner, when *describing structure*, makes no observation on the subject. The author of the Edinburgh Dissector holds the same opinion as Mr Hunter on this subject ; and in his excellent chapter on the teeth, although he does not disprove the opinions of Dr Blake and others, cautions the student against supposing Mr Hunter to be incorrect on this subject.

About this time the fangs of the milk incisives begin to be formed, in the accomplishment of which three contemporaneous actions are employed, viz. the lengthening of the pulp; the deposition of tooth substance upon it; and the adhesion to the latter of that portion of the inner surface of the sac which is opposite to it.

While the fangs of the milk teeth, particularly those in the front of the jaw, are lengthening in the manner now described, the pulps and sacs of the permanent teeth continue to increase, and the bony crypts which contain them to enlarge in proportion, the lower edges of the latter insinuating themselves between the two former. As this process continues, the jaw lengthens more rapidly, and when the infant is eight or nine months old, there is so much room in the alveolar arch, that the anterior permanent molar tooth begins to resume its former position in the posterior part of the dental arch, and the great cavity of reserve again to return to its original size and situation.

About this time the central incisives begin to pass through the gum,—a process which is accomplished in the following manner. The body of the tooth having been fully formed, and coated with enamel, has also been acquiring a portion of its fang, by the triplex action formerly described; in consequence of which, a reaction takes place between the bottom of the socket and the unfinished extremity of the fang. This reaction causes the body of the tooth and the non-adherent portion of the sac gradually to approach, and the former finally to pass through, the surface of the gum. Till the time that the edge of the tooth passes through the gum, the fundus of the sac, and consequently the base of the pulp with the extremity of the fang, never change their common relative position in the jaw. At the moment, however, that the tooth passes through the gum, (when the non-adherent portion of the sac resumes its primitive follicular condition, its inner membrane becoming continuous with the mucous membrane of the mouth,) the non-adherent portion of the sac shortens more rapidly than the fang lengthens, in consequence of which the adherent portion with the fang itself separates from the fundus of the alveolus, and the body of the tooth advances through the gum.* A space is thus left between the top of the alveolus and the fundus of the sac, occupied by cellular tissue, and traversed by the vessels and nerves. The alveolar cavity at the same time rapidly adapts itself to the new condition of its contents, advancing its edges so as to clasp the root, which has during these rapid changes been steadily lengthening,—a process which now goes on with greater rapidity, as it is conducted in a comparatively empty space. The pulp continues to lengthen till its base is no larger than the fasciculus of vessels and nerve which enter it. The orifice of the cavity of the tooth also diminishes to the same size, and through it the surface of the pulp becomes continuous with the adherent portion of the sac, and consequently with the mucous membrane of the mouth. The adherent portion of the sac has now attained its maximum, and the free or open portion its minimum size, having been reduced to that narrow portion of the gum which forms a vascular border and groove round the neck of the perfected tooth.†

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[*] Bell, Anatomy, &c. of the Teeth, p. 62.

[†] Hunter, Nat. Hist. Human Teeth, pp. 101, 102, 103.

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During the period that the milk teeth have been advancing along with their sockets to their perfect state and ultimate position in the jaw, the permanent sacs have been receding in an opposite direction, and have, as well as their bony crypts, been enlarging, the edges of the latter, insinuating themselves so far between the former and the milk sacs, that at last they are only connected by their proximal extremities, and ultimately, when the lower edges of the crypts sink so far as to have become the posterior lips of the alveoli of the milk teeth, the notches of communication between the latter and the permanent alveoli are forced, under the form of foramina, into a position on the anterior surface of the palate, one behind each milk alveolus. The sacs of the bicuspids having assumed a position directly above the milk molars, the hole of communication is never removed from the sockets of the latter.

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* The movement of the unfinished extremity of an incisive tooth from the fundus of its alveolus will explain what I have commonly remarked, and what must have been observed by medical practitioners, that from the time that the edge of the tooth appears through the gum, it advances more rapidly than can well be accounted for by the usual rate of lengthening of its fang. This advance is not invariably rapid, but may be observed in all the incisive teeth, if careful daily examination be made during a normal dentition.

† This vascular border may be seen in healthy gums which have not been disturbed by the deposition of tartar, and is beautifully displayed in two wet injected preparations, in the Bell collection, Museum of the Royal College of Surgeons, Edinburgh (Bell, C. iii. No. 25 and 56.)

It is interesting to observe, that one of the first physiological effects of mercury, viz. excitation of the gastro-intestinal compound glands and simple mucous follicles, is also displayed in a similar manner in the borders which surround the necks of the teeth, which are the remains of the free portions of the tooth sacs, while it at the same time acts upon the adherent portions and their submucous tissue, raising the teeth from their sockets, and affecting the jaw from contiguity.

The cords of communication which pass through these foramina are not tubular, although in some instances a portion of the unobliterated extrafollicular compartment of the original little cavity of reserve may be detected in them. They are merely those portions of the gum which originally contained the lines of adhesion of the depressions for the permanent teeth in the *secondary dental groove*, and which have been subsequently lengthened out, in consequence of the necessarily retired position in which the permanent teeth have been developed, during the active service of the temporary set. The cords and foramina are not obliterated in the child, either because the former are to become useful as "gubernacula," and the latter as "itinera dentium," or much more probably, in virtue of a law, which appears to be a general one in the developement of animal bodies, viz. *that parts or organs which have once acted an important part, however atrophied they may afterwards become, yet never altogether disappear so long as they do not interfere with other parts or functions.*

The sacs of the permanent teeth derive their first vessels from the gums; ultimately they receive their proper dental vessels from the milk sacs, and as they separate from the latter, into their own cells, the newly acquired vessels conjoining into common trunks, retire also into permanent dental canals.

It was stated above, that, in the child at the 7th or 8th month, when the central incisives were passing through the gums, the jaw had lengthened so much as to allow the first permanent molar to retire from the maxillary tuberosity, and to resume in some measure its position downwards and forwards in the same line with the other teeth, and also to reduce the great cavity of reserve to its primitive size. This cavity of reserve now begins to lengthen, to bulge out, and to curve backwards and upwards at its posterior extremity, under the form of a sac, into the mass of the maxillary tuberosity; a papilla or pulp appears in its fundus, and a process of contraction separates it from the remainder of the cavity of reserve, which still adheres to its proximal wall by one extremity, while by the other it is continued into the substance of the gum under the anterior molar. This new sac, which is that of the second permanent molar, now occupies the position in the maxillary tuberosity, which the first permanent did before it. It afterwards leaves this retired position, in consequence of the lengthening of the jaw allowing it to fall downwards and forwards into the line, and on a level with the other teeth.* Before it leaves the tuberosity altogether, the posterior extremity of the remainder of the cavity of reserve sends backwards and upwards its last offset—the sac and pulp of the wisdom tooth, which speedily occupies the tuberosity after the second molar has left it, and ultimately, when the jaw again lengthens for the last time, at the age of nineteen or twenty, takes it place at the posterior extremity of the range of the adult teeth.

The wisdom teeth are the second products of the posterior or great cavities of reserve, and the final effects of developement in the *secondary dental groove*.†

In the lower jaw, as in the upper, dentition commences in a deep narrow groove, situated between the lip and a semicircular lobe. This groove, instead of terminating in a simple curve posteriorly, as in the upper jaw, becomes shallow, and assumes a sigmoidal form upon the surface of the posterior bulbous ovoidal portion of the lobe.

About the seventh week the lip becomes very loose, and separates widely from the lobe, between which and the former a ridge appears, growing from behind forwards, and dividing the original groove into two, an outer one,—the labial duplicature of mucous membrane, and an inner,—the *primitive dental groove*. This ridge, which as in the upper, does not yet extend to the incisive portion of the jaw, is flat, or in the same continuous plane with the bottom of the dental groove, and its lip is turned out, or overhangs the labial mucous membrane. The inner lip of the groove is formed by the semicircular lobe, which has become thin, and arched over the groove, particularly anteriorly, where it is cut into four festoons, two on each side of the median line ; and posteriorly, where it still retains the appearance of an oval lobe, from under which the outer lip or ridge appears to proceed. The groove curves inwards between the two lips posteriorly, under a form which is evidently a developement of the original sigmoidal groove.

Near the posterior extremity of the groove there is an elevation of a small portion of its floor, which speedily becomes the germ or papilla of the inferior anterior milk molar tooth,—the second tooth which appears in the primitive developement of the human body. During the eighth week the elevation, already mentioned, becomes a papilla, lengthened from behind forwards, and flattened transversely. About the same time another papilla, bounded by a notched lamina, similar to those in the upper jaw, makes its appearance further forward in the groove. This papilla is the germ of the inferior milk canine. The dental groove is about the same time continued forward to the median line, not by the advancement of its outer ridge, but by the elevation of its floor. Its posterior portion also has become wider, and not so curved.

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* The curved lines which the posterior cavities of reserve, and the sacs of the molar teeth, describe in their progress to and from the maxillary tuberosity, and the coronoid process, and the peculiar position in which the pulps are consequently developed, explain satisfactorily certain normal and abnormal conditions of these teeth. 1. The curves which the combined grinding surfaces of the molar teeth present, convex downwards and backwards in the upper jaw, concave upwards and forwards in the lower. 2. The peculiar manner in which the fangs of the molars, particularly the inferior, are bent backwards. 3. The occasional horizontal position of the wisdom teeth, the crowns of the inferior being directed forwards, those of the superior backwards. This abnormal position is the cause of much annoyance and danger to the patient, and of difficulty to the surgeon.

† It is probable that the successive dentitions of the elephant are conducted in a cavity of reserve, which must consequently exist even in the adult animal, till a late period of its life. If such be the case, the molar dentition of the elephant, and the formation of the human adult molars, are analogous processes.

During the succeeding week the incisives make their appearance, the centrals first.

From this time all the eight papillæ continue to increase. The notched laminæ shoot inwards to the inner lip of the groove, near which they meet and join slight projections from it. About the eleventh or twelfth week the germ of the posterior milk molar appears in the curved portion of the groove, and is developed in the usual manner.

Crescent-like depressions appear in the secondary groove, on the inner side of the mouths of the milk follicles, as in the upper jaw.

The secondary groove adheres, leaving a posterior open portion, in which are developed the papilla and follicle of the first permanent molar. This follicle closes, as well as the lips of the portion of groove above it. There are now in the jaw ten milk tooth sacs, two permanent tooth sacs, ten anterior cavities of reserve, and two great or posterior cavities of reserve;[*] the ten anterior for the developement of the incisives, canines, and bicuspids; the two posterior for that of the second and third molar,[†] the coronoid process acting the part which the maxillary tuberosity did in the upper jaw.

SECTION III.

1.—*On the Division of Dentition into Stages.*—As dentition is a process, not only very complicated in its details, but of very lengthened duration, extending over nearly eight months of intra-uterine, and above twenty years of extra-uterine existence, the understanding and further investigation of it may be facilitated by dividing it into stages. The most natural division, one which is not artificial, but clearly indicated by the phenomena themselves, is into three stages, according to the position of the pulp in relation to its containing cavity; 1*st*, follicular stage; 2*d*, saccular; 3*d*, eruptive. We ought probably to consider, as anterior to the follicular, the papillary stage[*] during which the follicle or sac does not exist, and the future pulp is a simple papilla on the free surface of the gastro-intestinal mucous membrane. As this stage, however, is short in its duration, and simple in its details, it may be included in the first stage.

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[*] The mucous membrane constituting the cavities of reserve, exists in a condition which has hitherto been considered by anatomists as peculiar to the serous membranes. A dental cavity of reserve is a shut sac, lined by a true mucous membrane, which is isolated from the general mucous system, and performs no special function, till it is called upon to supply what it alone can afford, materials for the developement of a tooth.

[†] The cavities of reserve are occasionally somewhat undefined, two or three being conjoined, particularly posteriorly. Sooner or later, however, they become distinct. The great cavity frequently stretches forwards over the sacs of the milk molars.

The first or follicular stage comprehends all the phenomena which present themselves from the first appearance of the dental groove and papillæ till the latter become completely hid by the closure of the mouths of their follicles, and of the groove itself. It is upon this hitherto unknown stage of dentition that I have insisted so much in the former sections of this paper.

The second or saccular stage is the one with which anatomists have been so long familiar, during which the papillæ are pulps, and the open follicles which contain them are shut sacs, when the tooth-substance and the enamel, constituting the teeth themselves, are deposited. It is during this stage, also, that some of the most interesting phenomena in the formation of the alveolar processes present themselves.

The third or eruptive stage includes the completion of the teeth, the eruption and shedding of the temporary set, the eruption of the permanent, and the necessary changes in the alveolar processes.

When viewed in reference to an individual tooth, these three stages are distinct, but when viewed in reference to both sets, and to the whole process of dentition, they become somewhat intermingled.

When considered in the latter point of view, we may state that the follicular stage commences at the sixth or seventh week, and terminates at the fourth or fifth month of intra-uterine existence,—that the saccular commences at the termination of the first, and lasts for certain of the teeth till the sixth or eighth month, and for others till the twentieth or twenty-fifth year of extra-uterine existence, and that the third or eruptive commences at the sixth or eighth month, and lasts till the twentieth or twenty-fifth year.

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[*] Most anatomists have supposed the germs of the teeth to appear as shut sacs, full of a fluid, the pulps being formed by inspissation of the latter, or by developement from the walls of the former. Neither Mr Hunter nor Mr Bell have stated anything very definite on this subject. The pulp must be considered as the principal part of the organ, and as the element which appears first. The sac is a mere subsidiary part, supplied for purposes of developement and nourishment. Handbuch der Anatomie des Menschen, von H. Hildebrandt, besorgt. von E. H. Weber, Erste Band, p. 212; Handbuch der Entwickelungs-geschichte des Menschen von Valentin, p. 482; Arnold, Salzburg, Medicinisch Chirurgisch Zeitung, 1831, Erster Band, p. 236; Cruveilhier, Anatomie Descriptive, Vol. i. p. 518; Serres, Essai sur l'Anatomie, &c. des Dents, p. 59. Ph. Fr. Blandin, Anatomie du Systeme Dentaire, &c. p. 87; Blake, Essay on the Human Teeth, p. 2.

On the Anterior Permanent Molar Teeth.—The anterior permanent molar is the most remarkable tooth in man, as it forms a transition between the milk and permanent set. If considered anatomically, it is decidedly a milk tooth; if physiologically, a permanent one. In a former part of this paper, it was stated that the papilla and follicle of this tooth, were developed in a small portion of the primitive dental groove, which remained open for that purpose till the fourth or fifth month, while all the other permanent teeth were productions, not from the primitive groove, but from small non-adherent portions of the secondary groove, which lay in a level superior to the shut orifices of the sacs of all the milk teeth, and of the tooth in question,—the first permanent molar. In reference to its function, however, as the most efficient grinder in the adult mouth, we must consider it as a permanent tooth. It is a curious circumstance, and one which will readily suggest itself to the surgeon, that, laying out of view the wisdom teeth, which sometimes decay at an early period from other causes,* the anterior molars are the permanent teeth, which most frequently give way first, and in the most symmetrical manner, and at the same time, and frequently before the milk set.

On the Tardy Developement of the Superior Incisive Teeth.—A reference to the first section of this paper will show that at the ninth week, when the papillæ of the superior incisives are quite distinct, those of the inferior are with difficulty recognized. This is a fact which may be included under a law which will be more fully referred to afterwards, viz. that the dentition of the upper precedes, and is always in advance of the same process in the lower jaw. A week or two later, however, when the papillæ of the inferior incisives are imbedded and hid in deep follicles, those of the superior are nearly in their original condition. Although the latter recover in some degree their lost ground, yet, as every one knows, the inferior central incisive almost always cuts the gum before the superior, and the lateral sometimes does so also. In order to explain this apparent exception to the law above mentioned, it will be necessary to go a little into the history of the inter-maxillary bones, in doing which reference must necessarily be made to some of the other bones of the face and head.

When the superior portion of the large common nasal buccal and pharyngeal cavity is exposed in an embyro of the sixth or seventh week, by removing the lower jaw, we observe the boundary of the future palate to be defined by what has been denominated in a former section the horse-shoe lobe (*c*, Fig. 2.) Attached to the posterior inner edges of this lobe, two other lobes are seen. These grow from behind forwards, and from without inwards, and complete the palate by joining in the median line, being assisted in doing so posteriorly by two other smaller lobes, behind the posterior extremities of the horse-shoe lobe. In the two first lobes become developed the palatine plates of the superior maxillary bones, and in the two smaller posterior the palatine plates of the palate bones.

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* Bell, Anat. &c. of the Teeth, p. 183.

The bar (*h*, Figs. 2 and 4,) which ultimately coalesces below with the median line of suture of the four last mentioned lobes, is proved by developement to contain the nucleus of the vomer.

The median lobule (*m*) and its two lateral and anterior appendages (*n n*) form the anterior division of the embryonal palate. Of these three, the two lateral are observed in the course of developement to contain the nuclei of what are usually denominated the inter-maxillary bones. With regard to the median it may be stated that, as all the other lobules which appear in the soft pulpy texture of the fœtal palate, are proved by developement to contain the nuclei of all the well known bones of this region, I am inclined to consider it as indicative of the existence of the rudiment of a bone also, especially when the interesting antagonism, which I will show exists between it and the lateral lobules, is taken into consideration.*

As the object of this part of my paper, however, is not to discuss the osteogenesis of the human head, but to explain why the inferior incisive teeth, although later in their appearance, are yet more rapid in their progress than the superior, I shall now recall some circumstances formerly detailed regarding the developement of the three intermaxillary lobules, immediately before and for some time after the appearance of the incisive papillæ.

During the seventh week the three lobules are equal, and there is no appearance of either the upper or lower incisive teeth.

During the eighth week, the median lobule has increased relatively, and the laterals only absolutely; while as yet there is no appearance of either the upper or lower incisives.

During the ninth week the median has diminished relatively, and in the transverse direction; the laterals again have increased relatively and also in the transverse direction. This relative transverse increase of the lateral lobules is synchronous with the first appearance of the upper incisives. The inferior incisives are so indistinct at this time, as to be recognized with difficulty as slight bulgings on the floor of the dental groove.

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* A small cartilaginous body exists in the median inter-maxillary lobule of the child at birth. It is situated in front of the inferior orifice of the naso-palatine canal, and between the mucous membrane and periosteum.

The median inter-maxillary lobule exists in the adult palate, and may be felt behind and between the central incisives. Median inter-maxillary bones and cartilages exist in certain of the lower vertebrata.

The bar-like vomer of the human embyro at the sixth and seventh week, reminds the anatomist of the adult vomer of the lower vertebrata.

During the next fortnight, the relative size of the median and lateral lobules remains the same, and there is no further developement of the superior incisives. During the same period the inferior incisives have been rapidly increasing.

Afterwards the median undergoes much relative transverse diminution, while at the same time the laterals acquire a remarkable relative increase, which is accompanied by a corresponding developement of the superior incisives; but the inferiors have now got so much in advance as to retain their advantage ever after.

On the Laws which regulate the developement of the Pulps and Sacs, and the period of appearance of each of the Tooth Germs.—In the description which has been given of the earlier phenomena of dentition, it will be perceived that many of them range themselves under the laws recognized by MM. G. St Hilaire, and Serres, viz. the law of symmetry (loi de symmetrie,) the law of conjunction (loi de conjugaison,) the law of balancing or antagonism (le balancement des organes,) and the law of excentric developement (loi du developpement excentrique.)

The primitive and secondary dental grooves, the follicles, the cavities of reserve, the osseous alveoli of the milk teeth and their septa, are all formed originally of two halves, which ultimately join according to the laws of symmetry and conjunction.

The pulps of the milk teeth* with their notched laminæ are productions from the external lip or ridge of the groove. The interfollicular septa,—and the osseous alveolar septa, are also developed from without inward, (loi du developpement excentrique.)

I have already pointed out the beautiful example of antagonism which exists between the median and lateral elements of the intermaxillary system, and I may now point out, from among the facts formerly detailed, a few instances of the same kind, which must be referred to the same general expression, (loi de balancement.)

1.—Before the tenth week the upper lip is full and prominent, but at that time it begins to recede and gradually to disappear anteriorly, so as to expose the follicles and papillæ of the incisive teeth. It afterwards begins to regain its former position and size, and at the fourteenth or fifteenth week it is as large as the inferior, which from the first has not changed its appearance.

At the tenth week, when the lip begins to recede, the maxillary palate advances its anterior extremity, so as to conceal in some degree the intermaxillary palate (median and lateral lobules.) When the middle of the lip has disappeared, the maxillary has not only encroached upon the intermaxillary, but has also thrown itself into a bundle of irregular folds at its anterior part. As the maxillary palate retires, and the folds become regular crenated rugæ, the anterior part of the lip again appears, and at the fifteenth or sixteenth week, it is full and prominent, when the maxillary palate has retired to its proper position.

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* It is a curious fact, that the first tooth germs which appear, viz. those of the superior anterior and inferior anterior milk molars, are not productions from the external lip of the dental groove, but bulgings on its floor.

2.—When the outer lip of the primitive dental groove sends off the laminæ, which constitute the greater part of each of the interfollicular septa, and the floor of the secondary groove, the lip itself almost disappears.

The inner lip, again, which contributes a very small share towards the accomplishment of this process, becomes so much enlarged as to cover the whole groove.

3.—The external and internal lips of the primitive dental groove are, originally, equally prominent. The former, when it sends off the interfollicular septa diminishes, while the latter increases. When all the follicles of the primitive groove have been completed, the external lip begins to increase, and the internal to diminish. This increase of the external lip goes on after the closure of the secondary groove, until, at the fifth month, it becomes very prominent, and is divided into an incisive, a canine, and molar portion, each of which has a general similarity in shape to the acting portions of the corresponding divisions of the future tooth ranges. As long as it remains in this condition it is employed by the infant as a masticating organ. During this period the internal lip has altogether disappeared, except a small portion posteriorly; but a short time before the milk teeth appear, it again increases, and the raphe of the dental groove, instead of being hid behind the base of the external lip, is situated on the ridge of the dental arch, which now, as at first, is composed of two equally developed portions. The raphe forms a little border in the situation just mentioned, and is familiar to the eye of the surgeon, who, by its disappearance at any particular point, can satisfy himself of the proximity of the milk tooth under it.

Careful observation of the whole process of Dentition in man leads to the following conclusions:—

Milk Teeth.—1. The milk teeth are formed on both sides of either jaw, in three divisions, a molar, a canine, and an incisive, in each of which dentition proceeds in an independent manner.

2.—The dentition of the whole arch proceeds from behind forwards—the molar division commencing before the canine, and the latter before the incisive.

3.—The dentition of each of the divisions proceeds in a contrary direction, the anterior molar appearing before the posterior, the central incisive before the lateral.

4.—Two of the subordinate phenomena of dentition also obey this inverse law, the follicles closing by commencing at the median line, and proceeding backwards, and the dental groove disappearing in the same direction.

5.—Dentition commences in the upper jaw, and continues in advance during the most important period of its progress. The first tooth germ which appears is that of the *superior* anterior molar, which precedes that of the *inferior* anterior molar.

The apparent exception to this law in the case of the inferior incisive has already been explained.

Permanent Teeth.—6. The germs of the permanent teeth, with the exception of that of the anterior molar, appear in a direction from the median line backwards.

7.—The milk teeth originate, or are developed, from the mucous membrane.

8.—The permanent teeth, also originating from mucous membrane, are of independent origin, and have no connection with the milk teeth.

9.—A tooth pulp and its sac must be referred to the same class of organs as the combined papilla and follicle from which a hair or feather is developed, viz. bulbs. *

Explanation of the Figures.

a. Fig. 1. A tooth-germ—a bulging on a mucous membrane.

b. Diagrams illustrative of the three stages of dentition.

Fig. 1. Follicular stage.

Fig. 2. Saccular stage.

Fig. 3. Eruptive stage.

c. Diagrams illustrative of the formation of a temporary and its corresponding permanent tooth from a mucous membrane.

Fig. 1. Mucous membrane.

Fig. 2. Mucous membrane with a granular mass deposited in it.

Fig. 3. A furrow or groove on the granular mass. (Primitive dental groove.)

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* An abstract of this paper was read at the last meeting of the British Association for the Advancement of Science.

Dr Allen Thomson stated to me at that time, that he had no doubt that the fact of the *milk* tooth sacs being at one period open follicles had been observed, but that, then, he could not inform me where I could find it mentioned. I saw Dr Thomson in Edinburgh a few weeks afterwards, when, on looking into Valentin's work on Developement (Handbuch der Entwickelungs-geschichte des Menschen,) he pointed out to me the fact, that Arnold had observed that the milk tooth sacs were formed by a duplicature of the mucous membrane of the mouth, and had inserted a notice of the discovery in the Salzburg Med. Chir. Zeitung, 1831, p. 236. In order that Professor Arnold's discovery (which appears to have been altogether overlooked both in this country and in France) may be more generally known, I will give all his facts as he has recorded them. His notice occupies less than a page, and I am not aware that he has extended it elsewhere. At p. 236, loc. cit. he has observed, " in an embryo at the ninth week, we may perceive in both jaws, on the projecting edges of the gums, a proportionally pretty deep furrow, with ten depressions in it; a little later we may see a flat surface, on which there are many openings, communicating with small sacs, into which fine bristles may be passed. At the third month the sacs of the second molars may be seen communicating with the cavity of the mouth by small holes. The openings of the remaining sacs are soon closed by the mucous *membrane of the mouth.*

" *The sacs of the* permanent teeth are also formed immediately from the mucous *membrane of the mouth,* partly at the fourth month of fœtal existence, partly to-

Fig. 4. A papilla on the floor of the groove, (a tooth germ.)

Fig. 5. The papilla inclosed in a follicle in the bottom of the groove, (the latter in the condition of a secondary dental groove.)

Fig. 6. The papilla acquiring the configuration of a pulp, and its sac acquiring opercula. The depression for the cavity of reserve behind the inner operculum.

Fig. 7. The papilla become a pulp, and the follicle a sac, in consequence of the adhesion of the opercular lips. The secondary dental groove in the act of closing.

Fig. 8. The secondary groove adherent, except behind the inner operculum, where it has left a shut cavity of reserve for the formation of the pulp and sac of the permanent tooth.

Fig. 9. The last change rendered more complete by the deposition of the granular body, (the enamel organ of Hunter, Purkinje, and Raschkow.) Deposition of tooth substance commencing.

Fig. 10. The cavity of reserve receding from the surface of the gum, and dilating it at its distal extremity, in which a pulp is forming. Rudimentary opercula developing near its proximal extremity, and dividing it into a follicular and an extrafollicular compartment. Temporary tooth pulp nearly covered with tooth substance, and granular body almost absorbed.

Fig. 11. The cavity of reserve become a sac with a pulp, and further removed from the surface of the gum. Temporary tooth pulp covered with tooth substance, and granular body absorbed. (See Hunter, Nat. Hist. of Human Teeth, p. 95.

Fig. 12. The temporary tooth acquiring its fang by the triple action described in the paper, and its sac approaching the surface of the gum.

Fig. 13. The fang of the temporary tooth longer, and its sac touching the mucous membrane of the mouth.

Fig. 14. The temporary tooth sac again a follicle; free por-

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wards the end of that period, partly at birth. Once only, in a new-born child, I observed behind the most prominent edge of the gums, several openings which led to the sacs of the incisives and canines, and which are usually already obliterated before birth."

These are all the facts Arnold has recorded, and from them. it appears that he was acquainted at that time with the secondary dental groove, the ten milk follicles, and the ultimate closure of the latter. So far as we can judge from his brief notice, he appears to have been unacquainted with the mode of formation of the permanent follicles, supposing them to be formed immediately (unmittelbar) from the mucous membrane of the mouth, an opinion which is very prevalent among the continental anatomists. I can only account for the openings he mentions in the new-born child by arrest of developement, or by supposing that he had observed a few of the Tartar glands of Serres, (Glandes dentaires, Essai, &c. p. 28,) which are best seen at the period to which he alludes.

Having now mentioned all the facts which Professor Arnold has published, I may be allowed to state, that I had made out all the facts detailed in this paper, before I was aware that any of them had been on record; that I had given an account of them at the last meeting of the British Association, before I knew of Professor Arnold's notice; and that this paper was in the hands of the Editor of the Edinburgh Med. and Surg. Journal before I had an opportunity of seeing the Salzburg periodical.

I had also demonstrated the principle facts in the follicular stage of dentition in 1835, to Mr Nasmyth, to whom I am deeply indebted for the information he has given me respecting the anatomy and surgery of these organs, and in whose cabinet I at that time deposited preparations illustrative of the facts.

tion of the latter becoming shorter, and fang of the tooth receding from the bottom of its socket. Permanent tooth sac removing further from the surface of the gum.

Fig. 15. The temporary tooth completed. Free portion of the sac become the vascular border of the gum ; adherent portion become what is commonly denominated the periosteum of the fang, but which in fact is a triplex membrane, viz. mucous membrane, submucous tissue, and periosteum of alveolus or jaw bone. The permanent tooth sac much removed from the gum, but connected with it by a cord which passes through the foramen behind the temporary alveolus.

Fig. 16. The fang of the permanent tooth lengthening, and the crown approaching the gum. Fang of temporary tooth undergoing absorption.

Fig. 17. The same change more advanced.

Fig. 18. The permanent tooth appearing through the gum. Shedding of the temporary tooth.

Fig. 19. The perfected permanent tooth.

Fig. 20. The shed temporary tooth.

d. Diagrams illustrative of the formation of the three molar teeth from the non-adherent portion of the primitive dental groove.

Fig. 1. The non-adherent portion of the primitive dental groove.

Fig. 2. The papilla and follicle of the first molar on the floor of the non-adherent portion, which is now a portion of the secondary groove.

Fig. 3. The papilla and follicle of the first molar become a pulp and sac. The lips of the secondary groove adhering, so that the latter has become the posterior or great cavity of reserve.

Fig. 4. The sac of the first molar increased in size, and advanced along a curved path into the substance of the coronoid process or maxillary tuberosity. The cavity of reserve lengthened out or advanced along with it.

Fig. 5. The sac of the first molar returned by the same path to its former position. The cavity of reserve again shortened.

Fig. 6. The cavity of reserve sending backwards the sac of the second molar.

Fig. 7. The sac of the second molar advanced along a curved path into the coronoid process or maxillary tuberosity. The cavity of reserve lengthened for the second time.

Fig. 8. The sac of the second molar returned to the level of the dental range. The cavity of reserve shortened for the second time.

Fig. 9. The cavity of reserve sending off the pulp and sac of the wisdom tooth.

Fig. 10. The sac of the wisdom tooth advanced along a curved line into the maxillary tuberosity or coronoid process.

Fig. 11. The sac of the wisdom tooth returned to the extremity of the dental range.

ART. II.—*An Experimental Inquiry into the Physiological Action, the Poisonous Properties, and the Therapeutic effects of the Hydrocyanic Acid.* By HENRY LONSDALE, M. D. Member of the Royal College of Surgeons, London, &c.

I. *On the Physiological Action of Hydrocyanic Acid.*—IT is unnecessary to remind any one conversant with the history of the hydrocyanic acid, that much discordancy exists between the statements of those who have, or are said to have, pursued an experimental inquiry, to elucidate its physiological action. And since no agent can with confidence be applied therapeutically of whose effects on the healthy functions we are ignorant, it is almost superfluous to say that questions of doubt have arisen in the mind of the physician, upon the reputed medicinal power of this substance, on the grounds that satisfactory physiological data have not been furnished him.

Many deplorable sacrifices of human life, and a vast number of experiments on the lower animals, have fully established the accuracy of the announcement made by Schrœder of Berlin in 1802, that prussic acid is a poisonous agent. Three years after, Emmert's Inaugural Dissertation was published at Tubingen, containing numerous experiments and original facts.[*] Coullon, ignorant of what had been done in Germany, selected the same subject for his Thesis, when about to graduate at Paris, [†] and was amongst the first in France who observed the effects of this acid on vegetables. Ittner pursued the same investigation in Germany.[‡] M. Weicht [§] published an essay at Strasburg in 1813, in which he gave an analysis of what had previously been done by others. The inquiry now excited the attention of experimentalists, and more particularly in France; for it appears that no less than three separate essays [‖] were published in the same number of years.

On observing that great authorities were opposed in opinion to each other on one essential point, viz. the manner in which the vital functions are arrested, I deemed it requisite to test by a further appeal to experiment such of their statements as appeared to wear an aspect of contradiction or uncertainty. These experiments were, of course, made on the lower animals, which can afford only analogical evidence of the effects of a poison on the human species, however it may be remarked in support of the inferences I have drawn from such a source, that the greatest similitude is observed in the physiological action of the prussic acid on man, and some individuals of the class Mammalia.

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[*] Dissertat. Inaug. Medic. de Venenatis Acidi Borussici in animalia effectibus. Tubingae, 1805.

[†] Considerations Medicales sur l'Acide Prussique. Diss. Inaug. Paris, 1808.

[‡] Beiträge zur Geschichte ver Blausaure.

[§] Essai sur l'Acide Prussique, Strasbourg, 1813.

[‖] M. Robert, Annales de Chimie, Tome xcii. Oct. 1814; M. Gazan, Essai sur les effets del'Acide Prussique, Diss. Inaug. Paris, 1815; M. Callies. Essai sur l'Acide Prussique, Diss. Inaug. Paris, 1817.

The experiments are divided into three classes. 1. Those made with the diluted or medicinal acid of this country, which contained 3½ per cent. of anhydrous acid. 2. Those made with acid containing 12 per cent. of pure acid. 3. Those with the anhydrous acid. Twelve experiments were made with the diluted acid on dogs, cats, and rabbits.

Exp. 1.—A cat of moderate size was made to swallow twenty drops of the acid. It walked a few yards in a sluggish manner and then fell; in thirty seconds convulsions were observed, followed by slight emprosthotonos; the respirations were deep and slow, accompanied with a general trembling over the whole body. Before a minute had elapsed, the animal became insensible and motionless. The respirations were at long intervals; there were slight convulsions with an attempt to swallow, and urine was voided; the head was thrown backward; the animal ceased to breathe at the end of the second minute.

The chest was immediately opened, and the heart found contracting more feebly than natural. A vein in the vicinity was cut through, and no sooner had the blood poured out, than the contractions of the heart increased. As the blood flowed, these regular contractions became more active and forcible, until the end of the fifth minute, when the movements of the heart became irregular. At the tenth minute the ventricles were separated from the auricles and placed on the table; in two minutes, their contractions ceased, but the auricles left in connection with the blood-vessels continued to contract without the aid of a stimulus for at least four minutes after the ventricles had been removed. On pinching the brachial plexus with the forceps twenty minutes after the cessation of respiration, violent movements of the anterior extremities followed; the intercostal muscles contracted sometime after this even without the aid of a stimulus. The vermicular movements of the small intestines were observed an hour after the cessation of respiration.

Exp. II.—To a full-grown rabbit two drops were given. The animal was little affected, merely exhibiting a listlessness in its movements for a minute or so, and then recovered. Eight drops were now placed in the mouth, and in two minutes there was complete insensibility in all parts of the body with slight opisthotonos. This state continued for three minutes, when the hind-legs were observed to move, and the ears change in direction: in three minutes more apparent signs of recovery manifested themselves. The respiration and circulation were very little affected from the natural standard. Four drops were now thrown upon the eye, he staggered, and the limbs were thrown into convulsions, and after two or three shrieks he became insensible. Respiration soon ceased; yet the heart was felt through the chest beating for two or three minutes with some slight irregularities.

In the remaining ten experiments the same phenomena were observed as related above. In some the heart was scarcely at all affected, whilst in others, the contractions were arrested within five minutes after the cessation of respiration.

From the above experiments it may be safely inferred that the diluted or medicinal acid does not kill by arresting the heart's movements, or, in other words, does not produce death in the way of syncope. Since it is obvious that this acid does not exert any special influence on the heart's action, we look for some other explanation of the cause of death, which fortunately soon suggests itself on a due observance of the symptoms. That it must act on the central organs of the nervous system, the brain and spinal cord, is fully shown by the convulsive movements, vertigo, tetanic spasms, loss of sensibility and volition. And that coma is also the result of this action is readily apparent from the fact, that the respiratory acts are performed slowly and imperfectly. It is believed, then, that death takes place in the way of coma from the respiration being ultimately arrested; and although the circulation continues for some minutes scarcely unaltered, the venous blood gradually becomes impeded in its passage through the lungs, and consequently the further progress of this fluid to and from the right side of the heart is suspended.

The numerous experiments of Coullon and those of Emmert, Ittner and Gazan, as quoted by Coullon,* agree with those I have related; and the symptoms induced in wolves, bears, and vultures, by poisonous doses of prussic acid, as experimented upon by Jæger,† resemble in every essential point those observed in dogs and rabbits.

The Dublin Committee of the British Association appointed to ascertain the causes of the sounds of the heart frequently made use of the prussic acid in their experiments,—a sufficient proof that the movements of the heart continued after the respiration had ceased.

The poisoning of a great number of dogs at the police office of this city in the summer of 1835, showed that the heart's action was very slightly affected, and never *directly* arrested in these animals by Scheele's prussic acid, but only *indirectly* from the suspended respiration and consequent stoppage of the circulation of the blood through the pulmonary tissues. The large dose of an ounce was sometimes given.

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* Coullon, Recherches et Considerations Medicales sur l'Acide Hydrocyanique, p. 130, et seq. Paris 1819.

† Journal Complementaire, Tom. xxvii. p. 330.

Magendie, who ranks among the first of the experimental physiologists on the continent of Europe, appears very satisfactorily, (although some authors seem to think otherwise,) to support the inferences which I have been led to deduce from my experiments. It is evident from the quotation [*] given below, that this physiologist must have been operating with a diluted acid, and, allowing that he has somewhat exaggerated the time, "plusieurs heures," during which he saw such curious phenomena exhibited by animals dosed with acid, the positive statements he has made regarding the respiration and circulation cannot be called in question. As Magendie has made no mention of the means employed by him to prove the complete extinction of irritability and contractility, I am inclined to think, from collateral circumstances, that he had contented himself with watching the respiration and circulation, the animal having lost sensibility and power of volition, as witnessed in many experiments which I might detail, where doubtless he might suppose, from the quiescent state of the extremities, that the voluntary muscles had lost all contractility. The respiration is said to have continued easy; now it must assuredly be admitted, that the movements necessary to respiration could not be carried on without the aid of muscular contraction.

Orfila has remarked in his Toxicology, that first the contractility of the voluntary muscles, then that of the heart and intestines, is annihilated immediately, or a short time after death. [†] This statement, so opposed to all my observations, demands some notice. Let it be remembered that M. Orfila has copied verbatim from Coullon, nor does he add one single observation of his own; and yet he has hastily written, "it results from what has preceded," (viz. Coullon's own words) "and other facts that the irritability of muscular fibre is destroyed." What these other facts were, we are not informed. Lest it should appear I am dealing too harshly with the writings of this deservedly great authority in toxicological science, I will do him all justice by copying from the 157th page of his work, the following paragraph, which is evidently taken from Coullon.[‡] "Inspection of the dead bodies shows that the muscular organs, especially the heart, and in the latter almost always the right auricle and ventricle, remain long irritable; and the intestines are long agitated by the peristaltic motion."

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[*] "En étudiant les phénomènes de l'empoissonement par l'acide prussique, j'ai souvent observé des animaux qui n'offrant plus de trace de sensibilité, ni de contractilité musculaire locomotrice, conservaient pendant plusieurs heures une respiration facile et une circulation en apparence intacte bien que très accélérée, et qui, pour ainsi dire, etaient morts par leur fonctions extérieurs et vivaient par leur fonctions nutritives." Magendie, Recherches Physiol. et Cliniq. sur l'emploi de l'Acide Prussique, p. 8, or Annales de Chimie et Physique, Tom. vi. p. 351.

[†] Traité des Poisons, 3d ed. Tom ii. p. 165.

[‡] Coullon, Recherches, &c. p. 146.

Dr Granville favoured the profession, some time ago, with a volume on the Prussic Acid, leaving us to judge of its strength, by informing us that it was made at Gardens, in Oxford Street. It can only be inferred from the doses and medicinal use, that it was the diluted acid; yet how does he describe its action? "If the dose be large, and the animal in the full enjoyment of health and strength at the time of taking the acid, a sudden cessation of vitality takes place, quicker than thought can imagine, or words describe. It is as sudden as the cessation of motion in a watch when the finger is applied to the balance—yet none of the machinery is put out of order! No convulsions follow." * The statement, that there are no convulsions is, as far as I am aware, without a parallel in the history of the acid, and in no case of my observation was the animal free from convulsions. But Dr Granville in the succeeding sentence writes, "In some instances the animal whirls round once," which certainly implies something like disorder in the "machinery." It would be unprofitable to proceed further with Dr Granville's account of the physiological action of the acid, as the highly figurative language in which his expressions are couched, render them quite inapplicable to the purposes of a scientific paper.

2. Two experiments were made with the 12 per cent. acid.

An aged bitch swallowed twenty drops, and ceased to breathe within the minute. A large dog formed the subject of the second experiment. An ounce was poured down his throat: the contents of the bottle were scarcely emptied, when the animal cried out and experienced convulsions, and in less than twenty-five seconds respiration had ceased. The heart of both animals contracted regularly for three minutes. Allowing the blood to escape from the *vena cava,* renewed the contractions some time after respiration had ceased, in the first experiment, so late as the fifteenth minute. In the second experiment the heart was more decidedly affected.

It may be well to notice here the experiments of Schubarth, which have been reported by some of our British authors as showing that the contractility of the heart is destroyed by the hydrocyanic acid. The experiments, eleven in number, were made with Ittner's acid, which contains 10 per cent. of the pure acid. It is certainly true that Schubarth has in one place stated that the irritability of the heart is destroyed,† but on contrasting this with other remarks by the author, in the same paper, on the manner in which certain symptoms were developed, and death followed the administration of the poison, I am inclined to suppose that he has expressed himself too hastily in the passage to which we refer. For in page 77, he says, "if the animals die slowly, the first symptoms, after from fifteen to twenty seconds, were giddiness, then the breathing and action of the heart became exceedingly hurried." In a foot-note to the same page it is remarked, that in all cases the breathing or play of the lungs first ceased, then the heart moved for a short time, and when this ceased, there continued for a quarter of an hour an involuntary contraction of the muscles on exposing them, and peristaltic movements of the small intestines. And in alluding to the experiments where the dose was sufficient to cause immediate death, in which giddiness directly followed the administration of the acid; and the animals fell down senseless, had tetanic spasms, often gave a short but piercing cry, and then died, he writes, "Respiration completely ceased after from two to three minutes; the heart beat violently and convulsively about from four to sixteen minutes; and all signs of life then ceased."* Now, if the heart was primarily acted upon, as is stated in page 83, what are we to infer from the paragraph just quoted, in which we are told that the heart was acting violently and convulsively for two or three minutes. after respiration had ceased?

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* Granville, Historical and Practical Treatise on the Hydrocyanic Acid, 2d edit. p. 86.

† Bemerkungen über die Wirkungen der Blausaüre. Hufeland's Journal der Praktischen Heilkunde, lii. i. p. 83.

As the diluted acid is that which concerns the practitioner and the toxicologist, it would be unnecessary to make experiments with the concentrated acid, were they only to prove its fearful activity as a poison. It is, however, in reference to the action of the pure acid that so much altercation has arisen between British and foreign authors, and further experiments therefore seem necessary.

Magendie remarks of the pure acid, "that its activity is truly frightful, even to persons accustomed to observe the effects of poisons." † In support of this statement he relates the following experiment. A drop of the acid diluted with four drops of alcohol having been injected into the jugular vein of a dog, the animal "instantly dropped down dead as if he had been shot or struck by lightning."‡ In a preceding experiment he says it was impossible to find in the "locomotive" muscular organs any trace of irritability. May not Magendie in these experiments have taken the same test to prove the presence of irritability as in the numerous cases where he administered the diluted acid? Indeed, he admits in a foot-note written after the Memoir had been read to the Academy of Sciences, that he had frequently seen traces of irritability in animals poisoned by the pure acid. Nowhere in the memoir do I find this physiologist mention the effects of the acid on the heart, and Coullon concluded from a great number of experiments that the effects of the acid of Gay-Lussac in small quantities are similar to those obtained by a large dose of Scheele's acid.* M. Robert's experiments† merely point out the rapidity with which animals die when exposed to the vapour of the acid.

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* Bemerkungen, &c. Hufeland's Journal, lii. i. p. 79.

† Recherch. Physiol. et Clin. p. 4. or Ann. de Chim. et Phys. vi. p. 349.

‡ The expressions "quick as thought," frappe de la foudre, &c. &c. have of late become fashionable terms in medical literature. Such phraseology seems more adapted to astonish the vulgar, than to throw light on physiological phenomena.

We give Dr Christison's experiments in his own words. "In eight experiments on cats and rabbits with the pure acid, the heart contracted spontaneously, as well as under stimuli for sometime after death, except in the instance of a rabbit killed with twenty-five grains, and one of the cats killed by three drops applied to the tongue. In the last two, the pulsation of the heart ceased with the short fit of tetanus which preceded death; and in the rabbit, whose chest was laid open instantly after death, the heart was gorged, and its irritability utterly extinct."‡

Large doses of the diluted acid appear to act as rapidly as smaller doses of the pure acid, as the experiments of Macaulay and others at Leicester prove.§

3. The acid I employed in the third class of experiments was made according Gay-Lussac's method; by decomposing bicyanide of mercury with hydrochloric acid. The beak of the retort contained fused chloride of calcium and dry carbonate of lime: the receiver was kept quite cool. On Mr Kemp's authority the acid so prepared was perfectly anhydrous.

Exp. I.—Twenty drops were given to a middle sized cat. The animal jumped off the table, cried out, and became convulsed: opisthotonos succeeded, and the respiratory movements ceased within ten seconds. On opening the chest, the heart was observed to act more feebly than in the experiments with the diluted acid. Opening one of the large veins in the vicinity of the heart renewed its contractions. The voluntary muscles and intestines retained their contractility for several minutes.

Exp. II—VIII.—Three rabbits and four cats were poisoned by doses of from twenty to thirty drops of the same acid. These eight experiments with the strong acid offered the same results; the symptoms bearing the closest analogy to those observed to follow the administration of the dilute acid, with the exception of its acting more energetically on the heart in these last experiments. Being satisfied that the pure acid administered by the mouth, even in doses of twenty drops, though it enfeeble, does not destroy the irritability of the heart or voluntary muscles. I resolved to inject it into the trachea.

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* Coullon, Recherches, &c. p. 207.
† Annales de Chimie et Physique, xcii. p. 98.
‡ Christison on Poisons, third edit. p. 697.
§ London Medical Gazette, viii. p. 797.

Exp. IX.—Having exposed the trachea in the middle of the neck of a young rabbit, and made an opening sufficiently large to admit the point of a syringe ; nearly a drachm of the pure acid was injected. Before the syringe could be withdrawn the animal was affected with convulsions, which cast him with force from the table (3 feet high) upon which he was placed at the commencement of the experiment. He ceased to breathe at the seventh second. On exposing the chest, which was done very rapidly, the heart was seen enormously gorged on its right side, not so much so on the left. With great confidence in the renewal of its contractions by relieving the distension, I punctured the superior *vena cava ;* the blood spouted forth with considerable force, but there was no movement of the heart, not even of its right auricle, nor could any be excited on the application of a feeble degree of galvanism.

Before attempting to explain the cause of deviation in the phenomena observed in this experiment, from those already related, I beg to offer, with due deference to the authority of Magendie, my humble opinion, that no poison, however powerful, could act more rapidly, than the hydrocyanic acid did in this case ; as is sufficiently proved by the fact, which was observed by all present, that the rabbit was under its influence before the syringe was emptied of its contents. Notwithstanding this almost instantaneous action of the acid upon the nervous system, as indicated by convulsions, &c. the respiratory movements did not cease before the seventh second, and I am at a loss to understand how death could follow within a second of the adminstration of a single drop of the same acid.

Fom the sudden fall on the floor which the animal suffered, it appeared probable that the brain had suffered concussion, and thus influenced the contractility of the heart ; but in a second experiment, where the animal was held firmly on the table, and twenty drops thrown into its trachea, the same result followed, therefore the annihilation of the contractile property of the heart in these cases, may be fairly attributed to the effects of the acid. The only explanation that can be given, for the more destructive agency of the acid when thrown into the trachea, appears to me to rest on anatomical grounds. Since the epithelium, which lines the cavity of the mouth, extending along the gullet, and terminating in the human species at the cardiac orifice of the stomach, must be considered like the epidermis, an inorganic layer lying between the minute ramifications of blood-vessels, lymphatics, and nerves distributed on the mucous membrane, and the substance applied to these membranes, it is obvious that the action of the acid must be much more rapid when applied on the mucous membrane of the lungs, where, if the epithelium exists at all, it is so thin as not to be recognized by our senses, than when applied to such a membrane as that of the mouth.* The extreme thinness and great extent of the membrane which lines the minuter ramifications of the bronchial tubes, over the surface of which the acid must be rapidly spread on the first inspiration of the animal, affords another reason for its more powerful action. It is also possible, that the acid may be carried directly from the lungs, by the pulmonary veins, to the inner surface of the heart, and thus destroy its irritability.†

With a view to determine the quantity of acid which, thrown into the trachea, destroys the contractility of the heart, I poisoned two cats, three rabbits, and a dog with doses varying from one to fifteen drops. From these experiments, it appeared that any dose beyond ten drops injected into the trachea, will so influence the contractility of the heart as to suppress its movements within a minute. In some of these experiments we had an opportunity of testing in another way the effect of the strong acid on the system. On exposing the interior of the chest, the blood in the left cavity of the heart was observed to be imperfectly arterialized, and very shortly became quite venous. The venous blood now circulated in the system as well as the pulmonic heart. Whereas in those cases where the acid was thrown into the trachea, and the movements of the heart completely arrested, the left chambers of the heart contained purely arterial blood, and no change was observed to take place in its colour.

In imitation of Magendie's experiment, I made the following experiment. One drachm of 3 per cent. acid was injected into the jugular vein of a large-sized dog towards the heart, care being taken to prevent any loss of blood. The respiration ceased within a minute, but the heart continued to beat against the ribs for three minutes, and some time after this we observed its feeble movements.

What deduction, then, ought to be drawn from these experiments made with the strong acid? The experiments exceed in number, so far as I am aware, those performed by any single experimentalist in this country, and are, I hope, amply sufficient to enable us to arrive at some satisfactory conclusions, as to the action of this powerful substance on the animal economy. The accession of vertigo and convulsive action of the extremities, while the movements of the heart continue with a greater or less degree of regularity, distinctly show that the brain and spinal cord are directly affected. The diluted acid, as has already been shown, has an influence on the heart's action; but that exerted by the strong acid, as might be anticipated, is more marked and powerful. It must be distinctly understood, that when it is stated that the heart is affected, it is at the same time maintained, that the more direct action of the acid is on the sensorial functions. That the heart should be influenced, is not surprising when we remember its close connection with the central organs of the nervous system, and the facility with which its functions are disturbed by causes acting directly on the brain, as exemplified by the phenomena attending concussion.

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* Whether the acid produces its poisonous effects on the system by acting on the nerves of the part, or from being carried into the circulation, matters not in regard to this explanation; for whatever favours the one, is no less favourable to the other, as the same inorganic membrane which covers the blood-vessels and retards absorption must also prevent its application to the expanded extremities of the nerves.

† This is further confirmed by our observing that the contractility of the voluntary muscles and intestines are not more affected in these cases than where the dilute acid was given.

We do not expect any necroscopic appearances to prove a local influence of the acid on the brain, an organ which can only be affected from nervous or vascular connection with the part to which the poison is applied. It may also be remarked that, in some cases of simple apoplexy and other diseases terminating suddenly, in certain forms of jaundice, and *ischuria renalis*, where the bile and urea are not eliminated at their respective organs, but remain in, and are carried along with, the blood to the brain, and there act as poisons, exciting convulsions and coma, symptoms similar to those which follow the administration of the hydrocyanic acid, we find nothing on dissection indicating any disease of the cerebro-spinal axis.

Dr Christison states, with regard to two experiments which he performed, where the acid was given by the mouth in large doses, and destroyed the contractility of the heart, at the same moment that he believes idiosyncrasy may affect the results. As a farther explanation of these cases, I am inclined to add, what appears extremely probable, that some of the vapour of the poison was drawn into the lungs during a sudden inspiration of the animal.

From a careful review of the preceding facts, I am constrained to believe, that the immediate effects of the anhydrous acid are exerted on the brain and spinal cord, and that it also indirectly enfeebles, to a greater or less extent, the contractility of the heart. Cases may occasionally be met with (but I feel assured that they will be few in number) which may lead an experimenter to doubt the accuracy of my conclusions; but I think

I may venture to say, that, if he performs as great a number of experiments with the substance as I have done, he will be satisfied that such instances are rare, and must be ascribed to individual idiosyncrasy.

II. *Is Hydrocyanic Acid a Cumulative Poison?*—An interesting question connected with the physiological action of every poisonous agent is to ascertain whether it possesses a cumulative property, that is to say, whether, when taken for a certain length of time in small or medicinal doses, and without manifesting any morbid action, its toxicological effects may suddenly become developed, as is well known to be the case with some substances, *e. g.* mercury and digitalis.

A case is related by Dr Baumgärtner of Freyburgh, which is looked upon as illustrative of the cumulative power of the hydrocyanic acid. A man who had been taking ten drops of Ittner's acid daily, for a period of two months, without any poisonous effects being developed, was found one morning labouring under all the symptoms of poisoning with the acid. Tetanus and coma supervened. Venesection seemed to relieve the spasms and remove the tendency to delirium, but more than thirty hours elapsed before he recovered his sight.[*] The duration of the symptoms along with other circumstances render the case equivocal in reference to the subject under consideration, and the most probable explanation is, that the person had taken a larger dose than usual. It has been remarked by Dr Graves, that leeches soon perished when applied to the epigastrium of a person who had for some time previously been taking the hydrocyanic acid.[†] Such an occurrence, however, is no proof of the acid being the deleterious agent; for how often do these animals die when applied to the human skin, without our being able to discover the cause?

The great volatility of hydrocyanic acid is a property incompatible, in my opinion, with the power of accumulating in the system. For as the acid is absorbed into the circulation, it must soon arrive at the lungs, where, from the extreme tenuity of the membrane separating the blood in the serous capillaries from the air filling the pulmonary cells, it must escape from the body along with the carbonic acid in the expired air. This opinion, naturally adopted on theoretical grounds, is amply confirmed by experiment. In animals which lived some minutes after a fatal dose had been given, the air forced out from the lungs during the violent respiratory movements was so strongly impregnated with the odour of the acid, as to be perceived a few inches from the mouth of the animal; and from the odour being only sensible during expiration, it evidently did not depend on the state of the mouth. This statement was frequently verified, and many of my friends can bear testimony to its correctness. Moreover the same phenomenon has been observed in at least one individual of the human species poisoned by the prussic acid.*

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[*] Medizinisch-Chirurgische, Zeitung, i. 377. Quoted by Christison, p. 703.

[†] London Medical Gazette, xiv. p. 588.

That Baumgärtner's case is to be viewed as a casualty, seems obvious on the following grounds. It was not at all uncommon in Magendie's practice to administer the diluted acid for a period of two months and even longer without the appearance of any bad symptoms. The same might be said of many cases treated by British practitioners; and certainly the Italian professors would have observed cases evincing a cumulative property from the large and frequently repeated doses they have prescribed, had the substance possessed any such property.

III. *Medico-Legal Questions.*

After an elucidation of the physiological effects of the hydrocyanic acid, it seems necessary to allude, however briefly, to those circumstances which bear most directly upon legal medicine.

The examination of a medical jurist in a case of poisoning with this substance may be resolved into three questions; 1. What time elapses between the taking of the prussic acid and the first appearance of untoward symptoms? 2. Within what period does this poison prove fatal? 3. Are the symptoms induced in man by a poisonous dose of prussic acid always uniform and characteristic?†

Since the previous habits and idiosyncrasy of the individual,—the state of the stomach in regard to quantity and quality of contents, are all circumstances tending to modify the poisonous agency of the hydrocyanic acid, it is impossible to lay down general conclusions on the first of these questions. From a careful calculation of the ages, weight, &c. of several animals, and the doses administered to them as compared with man, I think it probable that a drachm of Scheele's acid would affect an ordinary adult within the minute, and a dose exceeding this, suppose three or four drachms, would exert its influence within ten or fifteen seconds. When the acid is stronger, and the quantity larger, we are pretty certain of its immediate action and consequent annihilation of the sensorial functions.

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* The case alluded to in the text is exceedingly interesting, as showing the successive stages of the symptoms produced by a dose, which, although large, was not destructive of life. Dr B. of Rennes took two teaspoonfuls of Vauquelin's acid:—in a few seconds he felt as if thunderstruck. An apothecary administered ammonia, but in small quantity, as the teeth were closed. The first symptoms were, insensibility, trismus, with noisy and rattling respiration, cold extremities, distortion of the mouth, and the exhalation of an odour of bitter almonds, small pulse, countenance and neck bloated and swelled, pupils fixed and dilated, in short, the state of an apoplectic about to die. He continued in this state for two hours and a half, at the end of which time his senses returned and he uttered, "J'ai pris de l'acide prussique, donnez-moi de l'air et laissez moi mourir." He then asked for coffee, but the irritable and ulcerated state of his mouth would not allow him to take it. A purgative lavement was administered, which produced five or six evacuations: at each evacuation by stool a quantity of gas, having the odour of the acid, was discharged from his mouth. No symptoms of paralysis. (Revue Medicale, 1825, i. 265.)

† This question has been entirely overlooked by English and foreign authors, (Beck, Christison, and others.) Its importance, however, is strikingly illustrated in the trial of Captain Donnelan for the murder of Sir Theodosius Boughton in the county of Warwick, England. The evidence of the late John Hunter is more particularly deserving of attention, for an abstract of which, vide Beck's Med. Jurisprud. 5th ed. p. 897.

2. As to the time at which the acid proves fatal? There are records of cases of the human species poisoned by the prussic acid where death occurred as early as the second minute, and as late as the forty-fifth minute. Should proper measures be adopted for the recovery of those poisoned, it seems probable that those who survive the fifteenth minute may recover. Most physicians will agree with me in thinking that the Parisian epileptics, if properly attended to, would have lived; and it deserves remark, that a smaller dose than that which they took is not likely to prove fatal. The dose amounted to 20 drops of the medicinal acid, which, allowing the acid to be of 3⅓ per cent. strength, would contain about two-thirds of a grain of pure acid.*

3. What symptoms are characteristic? The loss of consciousness and voluntary motion, the slow, deep, and heaving respiratory movements, whilst the circulation gradually becomes enfeebled, the fixed and insensible iris, in short, all the characteristics of apoplexy, were observed in the lower animals, and the same were observed in the physician at Rennes. It is far from improbable that a person may have a dose of prussic acid given him, so proportioned as to induce a simulated apoplexy, which, if the medical man was unaware of the cause, might be mistaken for the real disease. For it must be remembered that when doses not rapidly fatal are administered, the convulsions and tetanic spasms soon subside, and the animal lies in the apoplectic state for some time. So far then there is some difficulty in the way, and a knowledge of the general symptoms would be inadequate. But there is one circumstance which has received no attention from medico-legal writers, and that is the exhalation by the breath of the acid vapour. This has been observed in the human species, and numerous animals experimented upon by me, as already mentioned, and certainly would be sufficiently characteristic of the poison having been taken.

After a due attention to the symptoms, and the time and mode in which death takes place, it becomes necessary to detect by chemical analysis the presence of the noxious agent introduced into the animal economy. The small quantity of hydrocyanic acid necessary to prove destructive, its great volatility in an uncombined state, and facility of decomposition by substances with which it may come in contact in the stomach, are circumstances which have been, and always will be, great obstacles in the way of proving its presence.

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* Dr Geoghegan (Dublin Journal of Med. Science, Vol. viii. p. 309.) relates a case of poisoning, where about the same dose was taken, yet, by the application of remedial agents, the person recovered.

Much attention has been paid to the chemical means of detecting the presence of the hydrocyanic acid. The experiments of Leuret and Lassaigne are the most extensive. They found that in animals poisoned by small doses of the prussic acid it was not possible to demonstrate it by chemical means, after three days of exposure to the air after death.[*] Krimer states that he found the acid in the blood of an animal which died in 36 seconds.[†]

My chemical examination of the blood, brain, and contents of the stomach, according to the method recommended by Leuret and Lassaigne, leads to the following conclusion;—that from the fourth to the eighth day after death, although the odour is sufficiently strong so as not to be mistaken, yet careful distillation and the usual tests fail to prove the presence of the acid in animals which have been kept in the apartment where they were poisoned. These results coincide with what has been observed by Leuret and Lassaigne, who state that, when there was a strong odour of the acid in the brain and chest, they could not succeed in proving the presence of the acid by chemical analysis.[‡] The odour was very considerable in the contents of a stomach examined by Dr Turner, yet the quantity obtained by chemical analysis was very minute.[§] The person had taken a strong dose of the acid. In Chevallier's case, however, the poison was detected as late as the seventh day, although the trunk of the body had never been buried, but found lying in a drain.[‖] Gazan and Vauquelin state that the odour is sufficiently sensible in the stomach, although they could find no trace of the acid.[¶]

Various are the opinions which toxicologists have formed of the continuation of the odour after death. Magendie states that it is distinct for "several days."[**] Schubarth[††] observed the odour to be distinct in the blood, heart, lungs, and cavities of the brain for several hours after death, provided the animal died within from four to ten minutes. When the dose was so small as not to cause immediate death, it was found that not only a few hours after death, but almost immediately afterwards, no trace of the prussic acid could be found in the above-mentioned parts, viz. stomach, lungs, and brain. A horse died in thirty-two minutes. The dissection was made immediately after death, but no odour was distinguished.

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[*] Journal de Chim. Med. ii. 561.

[†] Journal Complement. xxviii. 10th experiment. I observe in the fourth conclusion which Krimer has evidently drawn from this experiment, that minutes are used instead of seconds, which we give in the text. It deserves mention that Krimer distilled the blood to dryness.

[‡] Annales d'Hygiene, Publ. iv. 422.

[§] Elements of Chemistry, 5th ed. p. 428.

[‖] Annales d'Hygiene Publ. ix. 337.

[¶] Coullon, Recherches, &c. p. 135.

[**] Magendie's Memoire, p. 6-7.

[††] Bemerkungen, &c. p. 84, 88.

Schubarth's experiments appear to show that, if life is prolonged beyond fifteen minutes after the poison is taken, we need not look for the odour. The bodies of the Parisian epileptics are said to have emitted no odour, which fact is seemingly in favour of Schubarth's statements. There can be no doubt that, if the animal survive some time after the administration of the poison, the acid may escape by the lungs or some of the excretions; but the data we possess at present on this point are insufficient to enable us to determine the time at which this may take place. It must, however, be remembered that Schubarth's dissections were made where there was a free current of air; indeed, he seems sensible of the fallacy arising from this source, by wishing to avoid such circumstances in future experiments. The temperature, situation, and exposure to which the body is subject during life and after death, require to be carefully considered as modifying the results.

With the view of ascertaining the duration of the odour after death, I caused several animals which had been poisoned by the acid to be kept in a room at the temperature of 50° Fahrenheit, during seventeen hours of the day, and others to be buried in light garden soil six inches below the surface. The results of these experiments (fifteen in number,) which were tested by at least three individuals in each case, lead me to expect the odour as late as the eighth or ninth day after death, even where life is prolonged to the eighth minute after a dose of the hydrocyanic acid.*

It has been stated by Coullon that animals poisoned by the prussic acid resist putrefaction. Emmert is of the same opinion.† Leuret and Lassaigne state that, on the exhumation of bodies (dogs and cats,) fifteen days after death, and even at the end of a month, putrefaction had not made very rapid progress.‡ The examination of human bodies and the fluid state of the blood in many cases reported by Coullon himself, do not support those who maintain the antiseptic powers of the acid. As it can only be by an extensive series of experiments that we can arrive at any thing satisfactory on this head, I refrain, for the present, from giving place to my limited observations.

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* From a review of the experiments alluded to above, I certainly believe that the odour is much less distinct where death is protracted for a few minutes, than where it immediately follows the exhibition of the poison, which to a certain extent corroborates the opinions of Schubarth. However, it was sufficiently sensible for at least twenty-four hours even in the case of a dog which lived twenty-five minutes, and in several others which lived a shorter time for four or five days.

† Coullon, Recherches, &c. p. 133. ‡ Journal de Chim. Medic. ii. 561.

IV. *On the Treatment of Poisoning with Hydrocyanic Acid.*—Impressed with the terrible rapidity with which hydrocyanic acid acts as a poison, many have argued that it is vain to think of antidotes, or any treatment that may be advised, proving sufficient to counteract its baneful agency upon the system. But several cases might be adduced where the action of the acid was more slow, and where sufficient time intervened between the exhibition of the poison and the fatal result to admit of the trial of restorative means. It is obvious from the nature of the poison that no remedial measures can be expected to prove beneficial unless they are energetic, and promptly had recourse to.

Ammonia is the antidote most generally recommended. Dr Mead appears to have been the first to observe its good effects.[*] Plenck ranks the *spiritus salis ammoniaci* amongst his antidotes to the cherry-laurel.[†] But the experiments which have led to its adoption as an antidote are those of Mr Murray and Professor Buchner. A dose of prussic acid, which killed one rabbit in ten minutes, was given to another, at the same time that ammonia was applied to the mouth on a sponge. The animal experienced no bad effects. From these and other experiments Mr Murray offered to take a fatal dose, provided some one stood by to administer the remedy.[‡] Experiments made with prussic acid on a cat, spiders, and flies induced Buchner to believe that ammonia was useful in restoring them from the convulsive stage.[§] Fremy[||] and Dupuy[¶] confirmed these experiments. Orfila formerly looked upon ammonia and all antidotes to the prussic acid as useless; more lately, however, he has made experiments, which are similar in result to those of Murray. Dr Herbst, whose experiments with the cold affusion will be presently noticed, says, ammonia ceases to be of service after the first doses, and is only useful in the first stage of symptoms or convulsions. From three experiments I have witnessed, I am inclined to support Dr Herbst in his opinion.

It is to be remembered that ammonia itself is an active poison, exciting inflammation of the Schneiderian membrane, in some cases causing bronchitis, and even death. A good illustration of its pernicious effects even when used as an antidote is to be found in the case of a physician at Rennes,[*] who laboured under inflammation and ulceration of the mouth for several days after recovering from the effects of the prussic acid. Such instances may well lead us to ask whether the poison or the antidote is more to be dreaded.

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[*] Mead's Medical Works, p. 131.

[†] Plenck, Toxicologie, 2d ed. p. 97. Vienna, 1801.

[‡] Edin. Philosoph. Journal, viii. 124. Mr Murray supposed the hydrocyanate of ammonia to be inert, hence his strong recommendation of this antidote. The experiments of Schubarth have shown that this opinion is incorrect, as the hydrocyanate of ammonia is scarcely less active than the prussic acid itself.

[§] Buchner's Repertorium für die Pharm. xii. p. 145–6.

[||] Journ. de Chim. Med. i. 482. [¶] Ibid. ii. 356.

Chlorine was first proposed as an antidote by Riauz, a chemist at Ulm, in the year 1822, who rested his recommendation on a single experiment made on a pigeon. Buchner confirmed Riauz's experiment.[†] MM. Simeon and Dauvergne,[‡] at the Hospital of St Louis, have also advised the use of chlorine. MM. Persoz and Nouat have been anxious to prove that the chlorine was absorbed into the circulating system, and there reacted on the hydrocyanic acid.[§] Orfila deems this remedy the most powerful antidote we possess.[‖]

If animals live to the fifteenth minute (as in the experiments related by Orfila, which are alone worthy of attention,) after a poisonous dose, it is probable that stimulants of a much milder nature may restore the animal to health, or, perhaps, in some instances, even spontaneous recovery might take place. Chlorine like ammonia is highly poisonous when used incautiously. Were this substance really an antidote, it would seldom be within reach in cases of emergency.

The cold affusion has been strongly recommended by Dr Herbst of Gottingen.[¶] His experiments appear to show the good effects of this practice during the "spasmodic period" of the symptoms. Dr Herbst believes that the effects of the acid, even when introduced into the organism in quantity more than sufficient to produce death, may be combated with success by affusions of cold water upon the body generally. Orfila speaks favourably of the cold affusion, but considers it to be inferior to chlorine. Dr Banks relates the case of a young girl (aged nineteen) who took forty drops of dilute prussic acid, and fell into a state of insensibility and convulsions. Stimulants were useless, but cold water poured from a pitcher on the head restored her to sensibility.[**]

From these experiments it appears that the cold affusion is a useful remedy in cases of poisoning with prussic acid. It certainly possesses advantages which few antidotes can boast of, viz. that of being always at hand and readily applied.

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[*] Revue Medicale. 1825, I. 265.

[†] Buchner, Repertorium für die Pharm. xii. 141.

[‡] Journ. de Chim. Med. v. 406.

[§] Anal. de Chim. et de Phys. xliii. p. 324.

[‖] Annal. d'Hygiene Publiq. Tom. i. p. 525.

[¶] Archiv. für Anatomie und Physiologie, 1828, p. 208, or Journal Complement. xxxiii. p. 13.

[**] Edin. Med. and Surg. Journal, July 1837.

Various antidotes have been recommended in addition to those just detailed. Ittner and M. Chancel* rely on the sulphate of iron united to potass. Oil of turpentine, introduced as an antidote by Emmert,† and at one time supported by Orfila, has been shown by the experiments of Coullon‡ to be quite inefficacious.

Though the value of those remedies commonly advised has been considered at some length, I am strongly impressed with the belief that an important part of the treatment of such cases still remains to be noticed. Looking at the mode in which death takes place,—the continuation of the heart's action for a short time after the respiration has ceased,—the gradually increasing distension of the right side of the heart by venous blood, and then the final arrestment of the movements of this organ;—the renewal of the contractions of the pulmonic heart after the withdrawal of a small quantity of blood from its auricle, are all circumstances of the utmost importance, as suggesting the principles of treatment It has already been stated, that the acid, although having a direct influence on the central organs of the nervous system, also affects the contractility of the heart, and that this effect on this organ varied from a slight diminution to complete suspension of its contractility, according to the amount of the dose and other circumstances. In those cases in which coma is induced, without much, if any diminution in the contractile power of the heart, the blood, notwithstanding, becomes congested in the right side of that organ, from the imperfect manner in which it is transmitted through the lungs, as occurs in all cases where the respiratory movements are imperfectly performed: bleeding from the jugular, by relieving the engorged state of the right side of the heart, may be expected to favour the remedial agent, such as dashing cold water upon the face, applying ammonia to the nostrils, &c. employed to revive the patient and cause him to breathe. It also appears that when the effects of the acid upon the heart are transitory, and produce only a temporary diminution of its contractility, the right side of the heart becomes engorged during the partial suspension of its movements, and that when it would renew its usual contractions, it is prevented from doing so by the mechanical distension of its walls. By opening the jugular, in these circumstances, we may hope to relieve the engorgement of the right side of the heart, and thus materially favour the renewal of its contractions. It needs no remark, that in those cases where the acid acts with sufficient energy to arrest primarily the contractility of the heart, all remedial agents will be applied in vain.

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* Dictionnaire des Scien. Médic. Tome xlv. p. 573.

† Buchner, Repertorium fur die Pharm. xii. 143.

‡ Coullon, Recherches, &c. p. 255.

I have often observed that the puncturing of the subclavian vein in the lower animals had the desired effect, and am therefore led, *à priori*, to believe, that the opening of the jugular vein in the human subject will serve the same purpose,—that of unloading the right side of the heart. But Haller has shown by experiment which was afterwards confirmed by Spallanzani, that the right side of the heart could be emptied of its blood by opening the jugular vein. This was attributed by Haller entirely to the derivation of blood, as is observed in the flow of this fluid from vessels opened at a great distance from the heart. These experiments of Haller have been more fully illustrated by my friend, Dr John Reid, in the 127th number of the Edinburgh Medical and Surgical Journal. He is of opinion that the flow of blood from the lower orifice of the jugular vein depends on the contraction of the right side of the heart; but in cases where the heart is extremely congested, he believes the derivation of Haller to come first into operation. Connecting Dr Reid's observations with what I have witnessed in thirty experiments, there appears little doubt in my mind as to the mode of proceeding in cases of poisoning with hydrocyanic acid. But as in practice we must be governed by more than theoretical opinions, experiments were instituted to ascertain the value of bleeding from the jugular vein. Before detailing these, it may be well to notice the opinions of authors. Hume observed that bleeding was useful in "relieving the violent symptoms" in a dog poisoned by the acid under consideration. Of its *modus operandi* we are not informed. Orfila repeated Hume's experiments, but without success.* Dr Paris says, "bleeding seems a decidedly fatal measure;"† while Dr Christison remarks, that "venesection is probably indicated by the signs of congestion in the head."‡ The discrepancy of opinion stated by the above authorities appears to be attributable to the want of physiological data.

Exp. I.—A ligature was passed around the left jugular vein of a small, lean dog, of the bull-terrier breed, five months old. The animal was made to swallow ten drops of prussic acid, § he became violently affected before the thirtieth second, and in ten seconds more there was complete tetanic rigidity of the whole body, with expulsion of the urine. As these two symptoms were very generally observed to be the immediate precursors of death, the jugular was now freely opened, and the blood flowed in a forcible stream to the extent of two ounces, when it was stopped.

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* Orfila, Toxicolog. General. 3d. ed. ii. 167.

† Paris and Fonblanque, Med. Jurisprud. ii. 406-7.

‡ Christison on Poisons, 3d ed. p. 715.

§ The acid employed in this experiment contained 4½ per cent. of the anhydrous.

As the ligature had been drawn tight before the vein was opened, the blood could only come from the larger venous trunks leading directly to the right side of the heart. After the bleeding, we felt more distinct contractions of the heart, and the respiration returned. In three minutes the respiration became more active, and there were some signs of returning sensibility. In ten minutes his state resembled that of drunkenness. Before the twenty-fifth minute he walked across the room; and in half an hour he was perfectly recovered.

In this remarkable experiment there was realized more than my most sanguine reliance on the treatment by venesection had ever anticipated. The dose, as proved by previous experiments, would have produced death in two, or at the farthest three minutes after being taken. That bleeding in this experiment was not beneficial by relieving cerebral congestion is quite apparent, because the ligature on the vein prevented the return of blood from this quarter.

In a second experiment, where a ligature was also placed on the jugular vein as in the previous experiment, bleeding was equally successful. It was observed in a third experiment where the dose of the acid was so large as to preclude all hopes of recovery, that bleeding from the jugular vein prolonged life, and thus afforded time for the application of other remedial agents.

The following experiment was made by Drs J. Reid and Duncan, and Mr Spence. To a large sized dog, a strong dose of diluted acid was given. The animal was soon affected with convulsions, which continued for two or three minutes, and he was apparently dying. The cold affusion had little or no effect. The external jugular vein was now opened, and the blood had scarcely commenced flowing, when the convulsive movements began to diminish, and continued to diminish rapidly, during the flow of the blood from the vein. After losing a few ounces of blood the animal recovered.

Dr J. R. Cormack has detailed a similar experiment, in which the effects of relieving the distension of the heart are strikingly illustrated. After the acid was administered the animal seemed to be in the agonies of death, but whenever the jugular vein was opened it began to revive, and seemed quite well at the end of an hour.*

From these experiments, I feel no hesitation in saying, what might indeed be inferred *à priori*, from an acquaintance with the physiological action of the poison, that bleeding from the jugular vein is of essential service in the treatment of poisoning with hydrocyanic acid. It evidently acts by unloading the congested cavities of the right side of the heart, which enables this viscus to renew its contractions until the coma subsides.

The following case, related by Magendie, is further confirmation of the utility of bleeding from the jugular vein. A young lady, eighteen years of age, through the neglect of her physician, swallowed a large dose of an extemporaneous mixture containing prussic acid. A few minutes afterwards, she was seen by Magendie, who found her labouring under drowsiness, convulsions, and "cerebral congestion." A large quantity of blood was abstracted from the jugular vein, and several drops of ammonia diluted with water were administered: these measures were followed by a sensible amelioration; consciousness and tranquillity returned.*

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* Cormack's Prize Thesis, On Air in the Organ of Circulation, p. 38, Edin. 1837. Dr C. has, in his Treatise on Creosote, (pp. 84 and 92, Edin. 1836,) mentioned analogous experiments in which creosote was the deleterious agent employed.

In my opinion, the relief of cerebral congestion was not what proved of advantage, for when a ligature was placed above the opening of the vein, as in two of our experiments, benefit resulted. There cannot however be the slightest doubt that bleeding is rendered still more efficacious by allowing some blood to come from the head.

V. *Therapeutical Properties of the Hydrocyanic Acid.*—To enter upon an historical account of the hydrocyanic acid as a therapeutic agent would serve no beneficial purpose. I shall therefore proceed to notice those diseases in which it has been recommended or found useful.

1. The practice of administering prussic acid in the diseases included in the order "Febres" seems entirely confined to the physicians of Italy, who recommend it on the same principle as the tartrate of antimony, viz. as a contra-stimulant. No one in this country, as far as I am aware, has adopted the Italian practice. From repeated trials of the medicine in hectic fever, Dr Granville speaks favourably. †

2. Professor Brera extols the acid in pneumonia, and it is probable that the large doses (above forty drops in the day) which he prescribed, might so far impair the sensibility as to "calm the movements of the heart and diminish febrile irritation." ‡ Dr Caspari recommends its use in the *phlegmasiæ* of the mucous membranes and glandular organs. He also relates several cases of puerperal peritonitis, cured by small doses of Ittner's acid every hour. § Cases of metritis are reported by Plattner, Dall' Oste, and Granville, as having been treated successfully by prussic acid. ‖ Dr A. T. Thomson is inclined to believe that, as the general sensibility is morbidly increased in phrenitis, the hydrocyanic acid may be useful as an adjuvant to other remedies. ¶ Dr Guerin of Mamers obtained beneficial results in two cases of brain fever. ** Caspari and his colleagues have treated gastritis and enteritis

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* Magendie's Formulary, translated by Gregory.

† Granville, Treatise, &c. p. 59.

‡ Prospetti Clinici, p. 29.

§ Rust, Magazin, xxii. 2e Cah. or Journ. des Progres des Scien. Med. ii. 63.

‖ Granville, Treatise, &c. p. 264, &c.

¶ Cyclop, of Pract. Med. Art. *Sedative.*

** Magendie's Formulary, p. 92.

with doses of hydrocyanic acid, and, as they say, successfully. As acute gastritis under an idiopathic form is exceedingly rare, it seems more probable that the medicine has been found useful in those chronic inflammatory conditions of the mucous membrane of the stomach;—cases which are not unfrequent in the higher classes of society.

A host of instances are brought forward by Granville to show the great utility of the acid in bronchitis, pneumonia and pleurisy. But since in all these cases we find the lancet to have been used, it is impossible to say how far the hydrocyanic acid assisted, if it assisted at all, in effecting a cure. There are cases of acute inflammation of the chest occurring, in which, after profuse bleedings and evacuants have not succeeded in arresting the disease, when the patient has become excessively weak and restless from want of sleep, and where depletion cannot be carried further with safety, that something of a soothing nature is required. Under those circumstances, the exhibition of the prussic acid may be attended with considerable advantages, and probably possesses a superiority over the strictly narcotic medicines.

Cases of rheumatism have been frequently treated by prussic acid. Mr Taylor ranks it as a specific.*

3. In hemorrhagies this medicine has been much lauded by Granville. "In *passive* hemorrhagies, no advantage,—in *active*, much confidence can be placed in the internal use of the hydrocyanic acid." † Dr Elliotson does not speak so favourably of its employment. ‡

4. *Stomach and Bowel Complaint.*—Under this head will be noticed the several forms of dyspepsia, and likewise dysentery, in which the hydrocyanic acid has been found useful. Dr A. T. Thomson was the first in this country to administer this medicine in cardialgia. He ascribed its good effects to its "reducing the morbid irritability of the secreting surface of the stomach, thereby enabling the juices of the organ to be more slowly secreted, and of a more healthy character." § Dr Granville has found it successful both in public and private practice. In 1820, Dr Elliotson published a pamphlet on the Efficacy of the Hydrocyanic Acid in Stomach Complaints; arranging these into classes. He relates seventeen cases where the chief symptom was "pain about the pit of the stomach." Many had tried the usual remedies with no relief. The acid effected a cure. This medicine relieved a variety of symptoms connected with the pain of the stomach, such as flatulency, loss of appetite, and depression of spirits, &c. Several cases of pain at stomach, attended with pyrosis, were treated with

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* Edin. Med. and Surg. Journ. xix. p. 399.

† Cyclop. Pract. Med. loc. cit.

‡ Cases by Elliotson, p. 49. § Granville's Treatise, p. 366.

equal success. From the reports of many of the above cases, it does not appear necessary that the bowels be lax or in a particular good state before the medicine can display its good effects. In four cases defined by Dr Elliotson, " in disorders of the stomach occasionally resembling some of the heart in the occurrence of sudden pain especially on motion, about the præcordia, and in the circumstance of the left shoulder and arm being affected," the alleviating power of the hydrocyanic acid was strikingly shown. The symptoms exhibited by those patients appear to me to differ little from those of *angina pectoris*; and if the pathological seat of this disease be proved to depend sometimes on affections of the stomach, as supposed by Drs Brera and John, * we might then hope to relieve those who unhappily suffer from this complaint. Dr Prout informed Dr Elliotson that the prussic acid failed only in one case out of several in which he tried it. † Dr Macleod recommends it strongly where there is much pain in the stomach, or anomalous feelings about the chest and heart. ‡ Many authorities and isolated cases might be quoted from the different periodicals of the day in proof of the efficacy of the acid in dyspepsia. My friend, Mr Hodgson of Carlisle, who has tried it in more than a hundred cases, remarks in a note to me, " This medicine seems to have a peculiar sedative action on the mucous membrane of the stomach, particularly when affected with that very common disorder, morbid irritability." As an apprentice, I have had frequent opportunities of witnessing the effects of the hydrocyanic acid in his hands; and although I omitted to take notes, I have a distinct recollection of one case in which the acid was eminently useful. It was that of a female relative who laboured for several weeks under pain of epigastrium, anorexia and vomiting, which were not at all relieved by the remedies usually prescribed. The hydrocyanic acid given in two drop doses two or three times a-day, soon enabled her to take a hearty meal without the least inclination to vomit. One of the best proofs of the acid being the remedial agent, is drawn from the circumstance of her having a relapse, which readily yielded to a few doses of the mixture previously employed.

Vomiting, a symptom arising from many morbid conditions, appears to be much relieved by the prussic acid. Excepting the strictly inflammatory states which give rise to this symptom, I am inclined to think, from numerous observations, that this medicine will be found highly useful in chronic vomiting, and may be ranked next to creosote in value. Several practitioners in London and elsewhere found it useful in arresting the vomiting which so invariably attended cholera.

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* Elliotson's paper, p. 33. † Ibid. p. 43.
‡ Lond. Med. and Phys. Journal, Vol. lix.

There can be no doubt but that pyrosis, and the other unpleasant symptoms associated with the different forms of dyspepsia, owe their origin to some particular state of irritability, over which the prussic acid exerts a speedy and remarkably beneficial influence.

The hydrocyanic acid has been recommended in dysentery by Dr A. T. Thomson, who attributes its good effects to the allaying of irritation. During the last summer I tried the acid in two or three cases with marked benefit. In one case, that of a female aged thirty, in her seventh month of pregnancy, affected with the ordinary symptoms of slight dysentery, I prescribed leeches, calomel, and opium, &c. with fomentations to the belly, for three days, with so little success, that the friends of the patient lost confidence in my recommendations. The prussic acid was now ordered, more with the view of changing the treatment than any great prospect of success, but in this I was agreeably disappointed, for on calling the next day I found the patient sitting up comparatively free from pain. When the uneasy griping sensations threatened her, she had recourse to two table spoonfuls of a mixture, which contained a scruple of Scheele's acid to eight ounces of distilled water.

As hypochondriasis depends on so many different causes, few practitioners will be found to support the statements of those who boast of the efficacy of the hydrocyanic acid in every form of the disease. Cases originating in derangement of the stomach, or kept up by morbid state of the chylopoietic viscera, may be advantageously treated by this medicine.

5. The violence of the paroxysms in tetanus has been allayed, and, according to Elliotson, a perfect cure of chorea established by the employment of the hydrocyanic acid. This medicine, like many other remedies prescribed in epilepsy, has now fallen into disuse, —partly from its inutility in the hands of numerous practitioners, and partly also from the recollection of the dreadful affair at Saltpetriere in Paris. Since epilepsy depends so frequently on organic affections of the brain, it is obvious that, in these cases at least, the prussic acid cannot be of the slightest advantage in arresting the disease.

Having pursued so far pretty closely the arrangement of Cullen, we may here, perhaps, advantageously deviate from this, and classify those diseases of the chest which cannot be so conveniently arranged under former heads.

6. *Some Catarrhal and Spasmodic Affections of the Respiratory Organs.*—With the exception of the purely inflammatory affections of the lungs, there are no diseases to which so much attention has been paid by the several writers on the prussic acid as catarrhal and spasmodic affections of the respiratory organs. The highly eulogistic strain of language in which many authors have

recommended its employment in those diseases can hardly find a parallel in the history of medicine. Brugnatelli recommended the acid so early as the year 1807, in angina, tubercular cough, catarrh, &c.* and Borda, Brera, and Heineken† have used it very generally in pectoral complaints. Magendie, who has perhaps investigated the therapeutical action of the prussic acid more than any physician in France, states it to be useful in the great number of cases of the nervous and chronic cough affecting females. In some cases where opium seriously incommoded the patient, and where no rest had been obtained for several days, the prussic acid calmed the cough in a few hours. Encouraged by this success, Magendie made repeated trials of the medicine in phthisis, and he states, that he found it " diminish the intensity and frequency of the cough, moderate and facilitate the expectoration, and in fine procure sleep without exciting the colliquative sweats." M. Lerminier, at the suggestion of Magendie, administered the acid in one-twentieth of the cases of phthisis admitted into the Hospital La Charité with great relief to the cough expectoration and watchfulness. Magendie seems fully confident of the curative effects of the medicine in the first degree of phthisis pulmonalis.‡ The Italian professors, according to Manzoni's thesis, have lauded the acid in every disease of the respiratory organs. Dr A. T. Thomson thinks the prussic acid valuable as a palliative in the early stages of phthisis, by " diminishing the hardness and frequency of the cough, and lessening the general hectic tendency." § Dr Nancrede of Philadelphia,|| Dr Kergaradec of Paris, ¶ and Dr Frisch of Nyborg,** confirm Magendie's statements. Dr Granville has devoted no less than a hundred pages of his treatise to cases of phthisis treated by himself and others, and seems to think that recovery may be expected even at an advanced period of the disease, if the acid be employed. It is well to remember, that at the time these reports of " incipient" and " confirmed" consumptive cases were drawn up, the stethoscope was almost unknown in this country, and must have been entirely so to Dr Granville, as may be seen by referring to the 206th page of his work It need hardly be remarked, that severe catarrhal affections of long standing induce very unfavourable symptoms, which might easily be confounded with phthisis.

Some authors are less sanguine of the utility of the prussic acid in phthisis, amongst whom is Dr Elliotson, who has frequently

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* Granville's Treatise, &c. p. 123.

† Nouveau, Journ. de Med. Aout 1821.

‡ Magendie's Memoire, *passim.*

§ Cyclop. Pract. Med. *loc. cit.*

|| Philadelphia Journal, May 1821, or Lond. Med. and Phys. Journ. Vol. xlvi.

¶ Magendie's Memoire, p. 59.

** Ibid. p. 92.

tried it even at the commencement of the disease, but with no success.

From the conversation I have had with a number of medical practitioners, I am inclined to suppose, that few physicians place much, if any, confidence on the acid in the treatment of catarrh and phthisis.

As medicinal agents may retard in some degree the first breaking up of the tubercular deposits in the pulmonary tissue, it is possible that the prussic acid may effect this purpose by allaying the irritability of constitution, cough, &c. which are usually present, at the same time attention is paid to the restoration of the healthy functions of the body by exercise and regimen. Should the tubercular ulceration be no longer a matter of doubt, we can only look for palliative remedies, and watch for any inordinate action of the circulation, which, if unsubdued, frequently leads to the fresh deposition and ulceration of tubercles. As opium and the strictly narcotic medicines frequently disagree with the patient, by affecting the head, exciting night-sweats, and disturbing the natural action of the alimentary canal, they are objectionable. On the other hand, the hydrocyanic acid, by subduing morbid sensibility, allaying cough, and promoting expectoration, without exciting the nocturnal sweats, becomes a useful palliative; but, as Dr Elliotson has remarked, "to extol it as a remedy is only doing an injustice to a most useful medicine."

The laurel water and free hydrocyanic acid are considered by Dr A. T. Thomson as almost specific in *phthisis trachealis.*

Opium often fails to relieve the paroxysms of asthma, and hence the numerous other narcotics recommended by practical writers on this disease. We have a mass of evidence in favour of the employment of the hydrocyanic acid in at least the spasmodic form of asthma. Granville recommends it in what he calls the "catarrhus suffocativus," and, according to Thomson, the acid acts almost spontaneously in the spasmodic asthma by relieving the oppressed state of the pulmonary circulation.* This it can only do by allaying the spasm upon which many of these alarming symptoms depend, and not from any peculiar action on the respiratory organs themselves. Magendie, Elliotson, and others speak highly of this medicine in the spasmodic form, where it certainly appears to be of great advantage. As asthma is frequently connected with, and aggravated by, organic diseases of the heart and lungs, we cannot expect any permanent relief from the acid in such cases.

Pertussis.—Dr Fontaneille, who saw much of this disease, and was amongst the first to try the prussic acid, states that he obtained wonderful results from the prussic acid.[*] He had tried the antiphlogistic treatment along with various narcotics, but without success. The disease was viewed by him as having its particular seat in the nerves of the mucous membrane of the lungs. Dr Kergaradec relates three cases.[†] In a review of Dr Macleod's cases in which the acid was employed, it is stated that, of upwards of one hundred cases of hooping-cough, only three proved fatal, the remainder having recovered under the use of the prussic acid, with the exception of twelve cases. [‡] Granville, Thomson, and Caspari [§] deem the acid to possess great advantages when early resorted to. As pathology has not as yet thrown much light on the proximate cause of hooping-cough, we can only trust to observation and experience in the recommendation of any medicine as a cure. In Fontaneille's cases there was no inflammation, and in twenty-three cases examined at Trevisa in 1816, the trachea was constantly found healthy, although lined with a frothy, blackish and adhesive mucus.

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* Cyclop. Pract. Med. loc. cit.

The hydrocyanic acid has been tried in colic, hysteria, and hydrophobia, but without much success, and although much lauded by Caspari, Taylor, and others, in neuralgia, it appears to be useful only where the deranged state of the stomach is the primary cause.

7. ***Skin Diseases.***—Caspari recommends the acid in erysipelas. According to Dr A. T. Thomson, it may be used as a lotion in three species of lichen,—in prurigo to allay the vexatious itching, and in lepra, where the skin is very irritable. In impetigo, where there is much vesication, and ointments are too irritating, the hydrocyanic acid properly diluted with the addition of the acetate of lead, was found highly serviceable. Ecthyma, eczema, and acme seemed also much benefited.[‖] Dr Schneider diluted the acid with alcohol, and used it as a lotion in prurigo and herpes. [¶] Dr Baganini employed it with great success in painful affections of the skin.[**] The experience of Dr Elliotson on the external application of the acid does not afford such beneficial results.

To give a summary of what has been written on the therapeutical properties of the hydrocyanic acid, it may be stated that this medicine is proved to be of some importance in certain forms of dyspepsia, in spasmodic affections of the respiratory organs, and in those cutaneous diseases attended with itching and irritability. That it may form a useful adjuvant to other remedies in allaying those of the troublesome symptoms of phthisis, in mild dysentery, where irritability is the principal cause, and in some neuralgic affections.

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[*] Description de la Varicelle qui a régné épidémiquement et conjointment avec la Variole dans la ville de Millau (Aveyron) en 1817. Montpellier, p. 21.

[†] Magendie's Memoire, p. 63. [‡] Lond. Med. and Phys. Journ. Vol. xlvi. p. 361.

[§] Journ. des Progres, loc. cit.

[‖] Atlas of Delineations of Cutaneous Eruptions.

[†] Rust's Magazin, quoted in Journal des Progres, Tome iii. p. 228.

[**] Bulletin General de Therapeut. Fevrier 1834.

The observations of numerous authors, combined with what I have myself frequently observed in the administration of the acid in dyspepsia, &c. lead me to believe that it is not necessary that the physiological action of the medicine should be induced before its therapeutic effects are manifested.

VI. *In what manner does Hydrocyanic Acid act as a Poison?*—This question, although of minor importance in practical medicine, is exceedingly interesting in a toxicological point of view. As it is only through vascular or nervous connection that a poison applied to one part of the body can produce its effects on the body generally, I may set out by stating that the hydrocyanic acid acts either by being taken into the circulation, and carried by that medium over the whole system; or by its direct impression on the nerves of the part to which it is applied, and so affecting sympathetically the other organs of the body.

However plausible the opinion may be, that the hydrocyanic acid acts on the system in the latter mode, or by *sympathy*, it must be admitted that not a few writers, even of authority on this subject, have adopted the former, or that of *absorption*. Many of the conflicting opinions on this subject are to be traced to the physiological doctrines in vogue at the time; others to preconceived notions of the authors; and not a few are derived from experiment. These latter alone deserve attention; but the limits of this essay allow only an epitome of the results of the experiments.

The experiment of Coullon, with which those of Emmert coincide, induced these observers to believe that the prussic acid does not act by means of the nervous system. The principal experiments were made on the brain, spinal marrow and nerves. Those on the brain are certainly objectionable, as part of the acid came in contact with the cut edges of the integuments. The second and third series of experiments proved nothing more than that a local action was exerted by the acid on the respective parts of the spinal cord and nerves, by which they were disabled from propagating (to the muscles they supplied) the impressions made on their poisoned surface.

These physiologists found that a tight ligature applied between the wounded part and the centre of the circulation, prevented the poisonous influence of the prussic acid. This is easily accounted for on the fact, that the limb was not in its natural state;—the circulation being obstructed, the nerves, even if not compressed, would have their functions annihilated for the want of due nutrition. Emmert's experiment of tying the aorta, and applying prussic acid to the posterior extremities without injury to the animal, is quite inconclusive. Coullon placed the cut ends of three branches of the brachial plexus in prussic acid without the animal experiencing any symptom of poisoning, which experiment certainly proves that the divided nervous trunks do not transmit the influence of the acid to the system.

The experiments of Krimer are the most extensive in support of the doctrine of absorption. From six experiments, he concludes that the acid does not act as a poison, neither when applied to the neurilema, nor to the nervous pulp itself; and that it exerts no greater influence on the brain and spinal cord than an equal quantity of alcohol. He cut the nerves going to the tongue of a dog, and applied the acid next day to this organ, and death soon followed. Apparently suspecting the fallacy of this experiment, he went still further, and not only divided the nerves of the tongue, but also the *par vagum* and sympathetic nerves in the neck of a dog, and then applied the acid as in the former experiment, and with an analogous result.

If Krimer had confined the acid to the tongue by proper apparatus, his experiment would have been conclusive, that absorption was necessary; but no mention is made of his having done so, therefore there is an evident source of fallacy. For allowing that all the nerves of the tongue were cut, have we not branches of the sensitive portion of the fifth pair distributed on the lining membrane of the mouth, and the inferior palatine coming from Meckel's ganglion, supplying the arch of the palate upon which nervous extremities the acid, from its great volatility, would soon come in contact, and induce its usual effects on the system ?

The decapitation of an animal, or removing the brain from its cavity, to try the effects of a poison when given by the mouth, as has been practised by Emmert, requires no comment.

Ligatures placed on the arteries of an extremity, whilst the nerves remain entire, preventing the poison applied to the limb from acting on the system, was to be expected from the imperfect vitality of the isolated limb. The reverse of this experiment, viz. the dividing of the nerves, and keeping the vessels entire, proves that the acid is carried by these vessels into the general circulation, but not that the poison acts in this way.

If the above experiments can be relied on, they show that the acid has no influence on the system, when applied to the trunks of nerves,—the spinal cord or brain. But do these facts militate against the opinion, now very general, that the prussic acid acts by nervous transmission ?

In reference to the nerves, it may be briefly remarked, that, from the mechanical arrangement of the filaments of the nervous trunks and expanded extremities, impressions made on the latter act more energetically than when applied to the former,—a fact which leads us to believe (what the above experiments go to prove) that the operation of poisonous agents is upon the sentient extremities, not the trunks of the nerves. The experiments made on the brain, instead of supporting the doctrine of absorption, are actually more in favour of sympathetic action ? Granting even that the blood was carefully wiped from the parts of the brain to which Emmert applied the acid, how is it possible that the acid could escape the mouths of the divided vessels, and be prevented from reaching the general circulation. Or if there were no open mouths, we should certainly expect it to permeate the coats of the blood-vessels themselves.

Having shown that there is a source of fallacy in the experiments which Coullon, Emmert, and Krimer instituted to prove the action of the prussic acid by *absorption*, it is necessary to state the facts which may be adduced in support of an opposite doctrine, or that of action by *sympathy*.

I shall commence by noticing those local effects of the poison, where no alteration of structure takes place in the part to which it is applied, and which are sufficiently well expressed by the term " nervous impressions." Robiquet observed, that when the finger was applied to the open end of a glass tube containing the vapour of the acid, it became benumbed, and remained so longer than a day. I have experienced a similar sensation, though in a slighter degree, from keeping my finger over the opening of a flask which contained the pure hydrocyanic acid. My friend Dr Howe, who kindly assisted me in many of my experiments, kept his thumb for some minutes upon the aperture of a flask containing three drachms of the strong acid. In a short time the sensation of numbness was perceptible, and continued for some hours. Professor Traill informs me that he has tried the same experiment several times, and with analogous result. Coullon's experiments on the hearts of frogs satisfactorily show the local action of the acid. The heart of one frog immersed in prussic acid lost its contractility entirely at the end of eighteen minutes, whilst another, left exposed to the air, continued contractile for several hours. He plunged one of the hind legs of a frog into the acid, and left the other free. The first became palsied in thirty five-minutes, whilst the other retained its contractility perfectly.*

Numerous are the instances recorded in works on toxicology, of persons who have been knocked down, or otherwise speedily influenced by a single inspiration of the vapour of the acid. The astonishing rapidity of the action of the acid in the experiments of Robert,† Macaulay,‡ Christison, and some of my own, appears incompatible with the opinion, that it must be absorbed before it an produce its effects, even supposing the investigations of Hering, which seem to show that substances are transported with great rapidity along the vascular system, to be correct. The single experiment made on the jugular vein, which I have detailed at page 47th, did not imply a more speedy action of the acid introduced into the system in this way, than by the trachea, which we should have expected according to the doctrine of absorption.

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* Coullon, Recherches, &c. p. 179.

† Annales de Chim et Phys. xcii. p. 98. The experiments were made with the vapour of the acid. Robert states that a bird had its beak applied to the opening of the matrass in which the acid was forming, and became instantly (" à l'instant") motionless. A rabbit and cat died almost as suddenly.

‡ *London* Med. Gazette, viii 797.

The experimental investigations of Morgan and Addison have elicited important facts in support of the sympathetic doctrine of poisons generally. At the same time I doubt much if these physiologists are correct in stating it to be *their own* theory,* if by this it is to be understood that it is entirely new. Fifty years have elapsed since Fontana was labouring hard to overturn the same theory, or rather, we should say, that view which Morgan and Addison are apparently inclined to adopt; for they have not explicitly stated whether it be the sentient extremities of the nerves found in the blood-vessels, or other parts of the body, that serve as the medium of communication between the poisonous agent and the brain. Fontana informs his readers, † that he found in some authors a great difficulty in favour of the influence of the nerves in opposition to the transmissile power of the blood-vessels, in the case of opium injected into the latter,—in the circumstance, that the action of the substance is conveyed suddenly in opposition to the nervous extremities of the red vessels even, and hence the whole of the nervous system.

Having stated both sides of this intricate question, and as far as my limits afford to an equal extent, I am willing to believe that, although the hydrocyanic acid is absorbed and carried into the circulation, its *modus operandi* upon the sentient extremities of nerves seems most probable.

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* Essay on the Operation of Poisonous Agents, p. 60. Lond. 1829.

† Traité sur le Venin de la Vipere, Tom. ii. p. 367. Florence, 1781.

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Art. III.—*Of a peculiarity of structure occasionally occurring in the Basilar Artery of Man.* By John Davy, M. D., F. R. S., Assistant Inspector of Army Hospitals.

Besides those peculiarities of structure of the basilar artery which are well known, there is one of not uncommon occurrence which, to the best of my knowledge, has not hitherto been noticed by any anatomist. It is a band in the interior of the vessel, attached to its side, and consequently intersecting it. It varies both in its dimensions and situation. I have most frequently found it near the junction of the vertebral arteries; very seldom near the commencement of the circle of Willis. Sometimes the band perfectly, at other times only partially, intersects the vessel. Sometimes I have seen it not more than one line thick, occasionally two or three lines. Its appearance, as regards its nature, has always been similar and most analogous to a fibrous structure. In every instance, I apprehend, it may be considered as congenital, and not the effect of disease. This is inferred after careful examination to endeavour to detect the effects of diseased action. In no instance were any indications of such action observed. The contiguous lining membrane was smooth, and there was no thickening where the ends of the band were inserted.

The basilar artery, in the manner in which it is formed, and in the thinness of its coats, may be considered as approximating to a vein. The similarity is increased by the peculiarity in question. Bands of the same kind are not uncommon in the longitudinal sinus, and more delicate bands and fibres are frequently met with in the heart in the right auricle, especially about the *fossa ovalis*, and in connection with the Eustachian valve.

Whether this peculiarity of structure has a decided use I am not prepared to say. In each instance in which a band or fibre presents itself, support is afforded, and additional strength is imparted. The band I have described, as occasionally occurring in the basilar artery, must necessarily have this effect.

It was in the month of June last year that my attention was first attracted to the subject. Since that time I have availed myself of every opportunity that has offered to examine the basilar artery. In ninety-eight *post mortem* examinations at which I have been present, made in the General Hospital at Fort Pitt, during the period, I have met with it in seventeen instances. In nine, death was owing to pulmonary consumption; in two to malignant tumour; in the remainder in each instance to a different disease. The ages of the individuals varied from 19 to 59; two were 25; two 35. In point of age there was no other accordance amongst them.

I shall give the results in a tabular form, and also the proportional frequency of marked difference of size of the vertebral arteries, to which my attention was at the same time directed.

Period.	No. of bodies examined.	Instances of band in basilar artery.	Instances of left vertebral larger than right.	Instances of right larger than left.
1837.				
June,	5	3	4	1
July,	16	6	9	1
August,	13	2	0	1
September,	9	1	2	3
October,	5	1	1	0
November,	8	1	0	1
December,	5		2	
1838.				
January,	6	1	2	
February,	3	0	0	
March,	1	0	0	0
April,	1	0	1	0
May,	8	1	1	0
June,	4	0	1	0
July,	5	0	1	0
August,	9	1	2	1
	98	17	26	8

In one instance of the total 17, a short filament proceeded from the intersecting band thus H; its end floating loosely.

In two cases out of the 98 there was another peculiarity, which I am not aware has ever been noticed, viz. an opening or foramen between the two vertebral arteries in the septum formed by their juxtaposition posterior to the basilar artery. In each instance it was sufficiently large to admit a surgeon's probe.

In three instances amongst the total number, each vertebral was as large as the basilar artery,—which was of the usual size of the artery,—and the vertebrals did not appear to be diseased.

The proportion in which the left vertebral artery was found larger than the right is so great,—viz. in the ratio of 26 to 8,—that it can hardly be considered accidental. But, on what the circumstance depends I am entirely ignorant. At one time, I supposed that it might be connected with the difference of origin of the right and left subclavian; but the notion was not supported by facts. In two instances (the only ones observed,) in which the left vertebral took its origin immediately from the aorta, between the carotid and subclavian, it was smaller than the right vertebral, taking its origin from the subclavian of that side, and that subclavian, as usual, from the *arteria innominata.*

In the table, I have given the results of my observations monthly for a particular reason, namely, for the purpose of showing how much more frequently the peculiarities of structure referred to, were presented in the bodies which died in one month than in those which occurred in another. The greater frequency of rare occurrences at one time than another is a circumstance extremely curious and mysterious; and I have often been impressed with it, as I believe others have been, both in hospital practice, and engaged in anatomical research. I shall mention a few instances from many, as examples of such impressions.

1. An oblique opening in the *fossa ovalis* of the heart, occasionally of such frequent occurrence as to give the idea of its being normal.

2. The Eustachian valve of large dimensions in the adult, nearly as large in proportion as in the fœtal heart.

3. A fibrous filamentous connection between the Eustachian valve and the auricular septum.

4. The mouth of the coronary vein destitute of a valve.

5. Delicate tendinous filaments or threads bordering the semilunar valves, especially of the aorta, giving the idea of an atrophied state.

6. The ligamentous cord, the remains of the *ductus arteriosus*, ossified.

7. Fibrinous concretions containing a purulent-like matter, formed during life in the iliac and femoral veins, and also in the ventricles of the heart, most frequently in cases of phthisis.

8. Varicose lacteals, also in phthisis.

9. Softening of the brain, especially of the fornix, in this disease.

10. Ulceration of the larynx.

11. Pneumathorax from perforation of the pleura in connection with a tubercular excavation.

12. Peritonitis, from perforation of the intestine, in connection with very limited local ulceration.

13. Ulceration and perforation of the *appendicula vermiformis*, and consequent peritoneal inflammation.

14. Softening and wasting of the articular cartilages, especially of the patellæ.

The list might be very much extended.

It may perhaps be said that there is deception in this matter, that the asserted greater frequency of occurrence of any peculiarity of structure, or lesion, independent of obvious causes, is more apparent than real, and that, were the same attention constantly given in search, the irregularity of this occurrence would cease. This may hold good in some instances; but I cannot admit that it is applicable in those I have enumerated, and more especially in the peculiarity of structure of the band in the basilar artery, which led to the remark. In each case the artery was carefully examined, and the result noted down at the time, and that specially. And, moreover, in relation to the subjects of observation, what could be less favourable for uniformity of result.

The individuals were not of any particular family or race; they were men of different regiments, English, Irish, Scotch, brought to the general hospital, the invaliding station of the army, from different parts of the globe, in a manner approaching to the accidental as nearly as possible.

I am disposed to believe that, were the pathological anatomist engaged in extensive and minute research, to institute a series of observations on organic changes analogous to that which Sydenham conducted in ordinary maladies, he might arrive at the conclusion, that there are organic constitutions prevalent at times, not less than atmospheric, and which (however produced) may be as much concerned in the origin of chronic disease, as the atmospheric influences are in the acute.

I shall give in a tabular form some of the results of my experience bearing on this subject, drawn from my notes of the various *post mortem* examinations which I have attended during a period of nearly eighteen years, namely, from May 1821, when I commenced the practice of making a note of every fatal case in which there was an examination of the body after death. My experience was chiefly confined to our military hospitals; indeed, at home it was entirely so restricted. On foreign stations it extended to the native population, especially in Malta, where the civil hospital offers an ample field for research.

Stations.	Years.	No. of bodies examined.	Instances of tubercles in the lungs.	Of ulceration of larynx with ditto.	Of ulceration of the larynx without ditto.	Of Pneumathorax.	Of varicose lacteals.	Of apertures in fossa ovalis
England, Fort Pitt.	1821	29	11	...				1
	1822	61	33	19			6	10
	1823	40	20	7		1		2
Ionian Islands	1824	10		2	1			
		18	5		1			2
	1825	36	2	2				1
	1826	9	3					
	1827	33	8	1				3
Malta	1828	6						
		32	5	1			1	5
	1829	19	4	1				3
	1830	41	5	1	1			9
	1831	54	19	3		1	2	8
	1832	55	8	1		3		7
	1833	49	17	9		1		5
	1834	57	15	5		3	1	5
England Fort Pitt.	1835	10	4	1				
		31	20	2		2		1
	1836	72	49	18		1		8
	1837	81	50	15	1	5		8
	1838	43	45	11		1		3
Total	18	786	308	108	4	19	10	82

On the results contained in this table, I shall restrict myself at present to a very few remarks.

The very large proportional number of cases in which tubercles were found in the lungs, viz. 39 per cent. of the whole, may excite surprise. I must confess it had that effect on my mind, and the more so, as no doubtful instances were admitted. I rigorously rejected every example not coming under the denomination of the consumptive tubercle—that is, a tubercle, albuminous in composition,—admitting of induration by boiling, as pointed out by Dr Abercrombie, and, of softening in the progress of disease, giving rise to vomicæ and excavations in the lungs. The melanotic tubercle was excluded, as also certain concretions more or less resembling tubercles, whether consisting principally of phosphate of lime and the other materials of bone, or of a nature approaching to cartilage. I may also remark, that no cases were admitted as supposed instances of tubercles, from the presence merely of cavities in the lungs. Cavities existing unaccompanied by tubercles were inferred to be examples of pulmonary abscess, of which several instances occurred.

No doubt, the astounding frequency of tubercles recorded in the table is partly owing to the description of cases sent to the General Hospital at Fort Pitt, where a considerable proportion of the whole mortality under observation occurred. But, making the most ample allowance on this account, I apprehend the conclusion is unavoidable, that the existence of tubercle is far more frequent than is commonly supposed, and the reported deaths from phthisis would indicate.

Of the four instances of ulceration of larynx unaccompanied by tubercles, the first was complicated with empyema and purulent effusion within the pericardium, to the extent of three pounds and a half;—the second was associated with melanotic tubercles in the lungs; and the third and fourth were connected with smallpox.

All the instances of pneumathorax, with the exception of one, occurred in cases of tubercular phthisis,—and originated in a communication of a valvular kind being established between the pleura and a bronchial tube by ulceration, commonly through the medium of a cavity. In the one exception, a similar communication was detected, the consequence of a partial destruction of lung from an abscess in the liver, penetrating and bursting into the lung through the diaphragm.

Of the large number of examples of aperture in the *fossa ovalis*, all were oblique, with the exception of three. Of the three direct, one was sufficiently large to admit the fore-finger, and two to admit the end of the little-finger. In neither instance was there the slightest appearance of the *morbus cæruleus*. The subject of the first mentioned and most remarkable example, was an old soldier, who for many years enjoyed excellent health. In these cases, it appears inevitable that there must have been an admixture of venous and arterial blood in the auricles. In the examples of the oblique passage, the probability is, that no blood flowed from one auricle into the other. In one instance, in which the oblique aperture was sufficiently large to receive the end of the little-finger, the right cavities of the heart were found distended with coagulated blood, and the left empty.

Fort Pitt, Chatham, August 24, 1838.

Art. IV.—*A Case of Fibrous Tumour of the Uterus terminating fatally three weeks after Delivery, with observations on diseases of this character.* (Accompanied by an Engraving.) By J. T. Ingleby, M. R. C. S., Lecturer on Midwifery, Birmingham.

Mrs P. having been previously in perfect health, and never having been the subject of any uterine malady whatever, was delivered of her fourth child on Wednesday evening, November 22. The removal of the placenta was followed by hemorrhage of slight amount, yet producing faintness and much depression of the system. On Thursday and Friday she vomited and was faint; and it is material to observe, that, on one or both of these days, she was harassed by a sense of bearing down and tenesmus, and expressed a conviction that there was another child in the womb; but the uterine tumour did not appear larger than natural. On Saturday morning the pulse had become frequent, averaging from 120 to 130, and in the course of the day the abdomen became painful and was tender when pressed. I visited the patient on the evening of Sunday, in consultation with Mr George Elkington, by whom she was attended.

She still complained of the abdomen being painful, and the region was not only tender to the touch but tumefied and puffy. She also complained of an occasional attack of pain in the lumbar region, attended by tenesmus. The pulse ranged from 120 to 130; the respiration was quick; the countenance very pale; and she had excessive thirst. The alvine dejections were discharged frequently, and had the appearance of yeast, but subsequently they assumed a more healthy character. Ultimately, however, an intractable kind of diarrhœa took place, tenesmus being a most harassing feature throughout the whole course of the disease. Vomiting every now and then occurred. The tongue was rather coated, but not otherwise unhealthy. The lochia were very scanty;

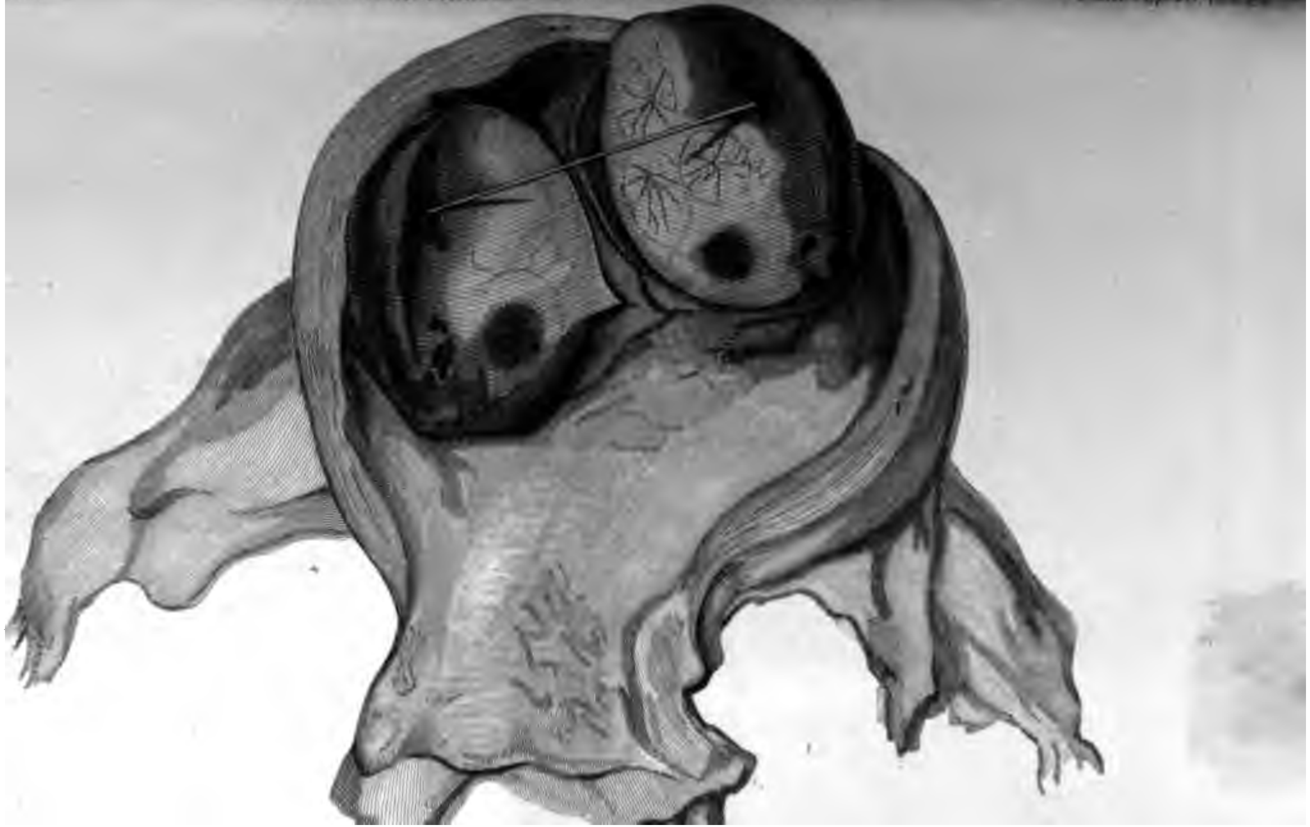
indeed there was scarcely any uterine discharge from the time of her delivery, excepting the slight hemorrhage already adverted to, and a large serous discharge tinged with blood, which escaped shortly only before death. There was no secretion of milk. The intellect was for the most part unclouded, but several transient attacks of delirium took place during the progress of the complaint. There was no rigor at any period. The pain and tenderness in the abdomen soon gave way after I saw her, and, on the occasion of my second or third visit, pressure afforded her so much relief as induced us to recommend the application of a flannel bandage.

Opium and calomel, with saline medicines, constituted the early part of the treatment; but the depression which prevailed throughout the greater period of the complaint, was such as called for the administration of quinine and ammonia with diffusible stimuli, generally, and as large a supply of nutriment as the stomach would bear. The exhaustion nevertheless continued increasing, and death occurred on the 21st day after her delivery.

The symptoms were exceedingly ambiguous and perplexing. At the onset of the disease we feared peritonitis in its ordinary form; presently we thought we had to encounter an obscure kind of puerperal fever. Doubting, therefore, the real nature of the attack, we were more than usually anxious to obtain an examination of the body, and succeeded in our object.

Inspection.—The intestines and the viscera of the abdomen, in general, presented a preternaturally white appearance, and there was neither lymph nor serous effusion within the peritoneal sac. The uterus was scarcely larger than is usual at the period which had intervened from the delivery, and its mouth was nearly closed. The cavity on being exposed was observed to be nearly filled by a tumour the size of an orange, attached to the posterior surface of the centre of the fundus by a broad basis, but not reaching the orifice of either Fallopian tube. Its anterior and exterior surface presented a soft, irregular, and broken down appearance, having clots of blood in the interstices; but this appearance was limited in depth to about a third part of the mass; for, on making a complete section, its structure was found much firmer and evidently fibrous, presenting as it approached the uterus a white appearance, but evidently, in the course of softening, by the supply of blood it received during gestation. On dissecting away the cellular tissue which connected the basis of the tumour with the uterus, a cyst was distinctly seen from which the mass could be separated without difficulty, and the hollow in which it lay imbedded was exposed to view. The projecting sides of the mass presented a somewhat irregular surface, and at one point traces of lymph were visible. A number of open-mouthed vessels were seen on the surface of such parts of the tumour as approached the walls

Surg. Journ. VIEW OF THE FIBROUS TUBERCLES IN [illegible]'S CASE.



of the uterus. In various parts of the lining membrane of the uterus well defined ulcerations were distinctly visible, being about the size of a split pea, of a circular form, and not extending below the lining membrane.

Observations.—We have in this case decided evidence of the changes produced in a fibrous tumour under the excitement and impulse of pregnancy. The existence of the tumour was not even contemplated, and previously to the last conception, its bulk could probably not exceed, if it equalled, that of a walnut. Originally imbedded just beneath (I presume) the lining membrane, its growth would be greatly promoted both by the influx of blood and the laxity of the surrounding structures, and occasion the mass to project in that particular direction where there was the least resistence; and we at once understand how it came to occupy the uterine cavity. The placenta must necessarily have covered it; a circumstance to which the soft broken down appearance, together with the blood extravasated under its exterior surface, may be attributed. The pressure of the placenta on the one hand, and the increasing growth of the tumour on the other, may also account for the complete absorption of the mucous coat of the uterus over the tumour, its surface being destitute of covering. The presence of lymph upon one side of the tumour, and the ulcerations in different parts of the lining membrane of the uterus, are sure evidences of a pre-existing inflammatory process which, in this instance, must have followed the delivery, and produced the irritative fever and the fatal result.

The circumstances of this case afford another proof of the inability of a fibrous tumour, confined within the coverings of the womb, to withstand the changes consequent on conception and delivery; for, however previously slow in growth and languid in action, too insensible and, perhaps, too small to excite attention, it receives a sudden impulse, it grows rapidly, and, should inflammation not be induced during the term of pregnancy, parturition and its effects are greatly to be dreaded,—directly as respects hemorrhage, indirectly as respects inflammation and its consequences, namely, softening, ulceration, suppuration, absorption of the contaminated fluids, fever, and a fatal result. The difficulty of forming a correct diagnosis, as well as the danger incident to a disease of this character when complicated with pregnancy, may be clearly deduced from the circumstances of this case.

But an interesting and most important inquiry presents itself,—what would have been the effect of inducing labour prematurely? Somewhat doubting the maturity of Dr Ashwell's views relative to this particular measure in cases of pregnancy, complicated with fibrous tumour,[*] (the supposed necessity being founded upon severe constitutional symptoms having appeared in a preceding pregnancy,) I ventured to express these doubts in a recently published work.[†] Dr Ashwell replied to my remarks, and reiterated his former views in a way which demands my most respectful acknowledgement.[‡] My object, in common with Dr Ashwell's, is not to resist conviction, but to establish correct principles and to elicit truth. But this case cannot be said to sanction the practice which Dr Ashwell has so ably advocated; for, admitting that the disease must have made great progress during pregnancy, there was not a single symptom antecedent to delivery by which its existence could be known. Had premature labour occurred *spontaneously*, whilst the tumour was in a less advanced state, probably the symptoms would not have appeared. But in resolving to induce labour *artificially* and *prematurely* in diseases of the womb, many important circumstances, exclusive of the moral justification, have to be considered. It is satisfactory, however, to know, that although a fibrous tumour may be attacked with inflammation soon after delivery, the changes may be of a far more salutary kind, for the tumour may undergo a great reduction in size, to which a copious lochial discharge is peculiarly conducive. In support of the principle I annex the following case.

About a year and a half ago, I was requested to see the wife of a most intelligent country practitioner, under these circumstances. The lady, who was in the fifth month of her seventh pregnancy, had experienced at different times in the course of two years very uneasy feelings in the womb and especially so after exertion. The sixth pregnancy was attended with suffering, and the delivery was followed by repeated discharges of blood and the expulsion of coagula of a chocolate colour. These symptoms continued during some weeks of the seventh pregnancy, accompanied by a throbbing and burning sensation in the uterus, aching in the back and outside of the left thigh, and sometimes with numbness and inability to use the limb with freedom. On examination both *per vaginam* and through the abdominal coverings, I discovered a tumour as large as a moderately sized fist, firm and tender to the touch, occupying the left groin underneath the peritoneal coat of the uterus, and corresponding to the anterior and lateral part of the cervix. The tumour was so very painful whenever the patient stood or walked, that she was obliged to lie down the remaining part of the pregnancy, every means being at the same time enforced, calculated to remove or prevent inflammatory action. Under this treatment there was no sensible increase of the tumour; there was less hemorrhage than usual at delivery; and at the end of the second month the tumour had so very nearly disappeared, that I could barely distinguish a little irregularity, and shortly afterwards not even this. The lochia, or a discharge of a muddy red colour, continued many weeks after delivery; a circumstance which I think contributed much to the favourable changes. She was not suffered to nurse the infant. I directed the husband's attention to Dr Ashwell's opinion in the event of the symptoms increasing during the pregnancy, so as to bring life into danger.

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[*] Guy's Hospital Reports, Nos. 1 and 2.
[†] Facts and Cases in Obstetric Medicine, page 155.
[‡] London Medical Gazette for December 17, 1836.

I believe it is held as a general rule, that a fibrous tumour in the womb, however accessible to the finger, ought not to be removed by operation during the puerperal state, both on account of the increased liability to *peritonitis* which attends this state of the system, and other obvious reasons. Had life been preserved in the case first mentioned, the situation of the tumour in the cavity of the uterus would probably have favoured its ultimate developement, and an attempt to remove it either by the knife or ligature might have been necessary. Its growth, however, must have had the effect of dilating the lower part of the womb and facilitating its removal. But it is my intention to trouble you with some plain remarks on the treatment of complicated tumours of this character in a future number of your Journal.

Respecting the more complex class of tumours, whether of a fibrous, fibro-cartilaginous, or sarcomatous character, connected with the uterus by an extensive basis, and remaining wholly, or in a very great measure within its cavity, our information is still defective, and our practice indeterminate.

The distinguishing features between a genuine polypus and a non-pediculated growth having white lines, and not susceptible of being injected, are by no means clear and satisfactory; and since there is every variety in the structure of a pediculated polypus —since some of these growths intimately resemble the structure of the fibrous tumour—the difference does not appear to me to constitute a distinct disease. Much, it is thought, depends upon the original situation of the growth as well as its progress and vascular connection; and although Dr Ashwell cannot believe that "a genuine fibrous tumour ever becomes a pediculated polypus," I am quite convinced that a fibrous tumour may ultimately descend sufficiently low to admit of a ligature being applied, just as in an ordinary case of polypus. I have seen this myself, and stated the case.

Dr Ashwell informs us, that, in thirty examples in Guy's Hospital Museum, there are only three or four where the growth encroaches on the uterine cavity,—a very striking fact; and yet at the present moment I have several patients in whom the uterine cavity is perceptibly encroached upon by fibrous degenerations. A brief allusion to a case of recent occurrence may not be uninteresting, both in reference to the origin and course of uterine tumours, and as giving support to the opinion, that tumours developed in the proper tissue of the uterus are determined in their final character according to the direction they ultimately assume. A woman died from peritonitis a few days after the removal of a polypus by ligature. On examination after death, a number of small conical-shaped tumours was discovered in many parts of the womb; some of these had reached the mucous membrane, some had reached the serous membrane, and projected very visibly in each direction.

A fibrous tumour imbedded within the walls of the uterus, and forming a large tumour in the hypogastric region, is but little under the control either of art or nature. This is especially true when the tumour projects towards the peritoneum; but when it projects in the opposite direction, it is possible that the body and neck of the womb may yield, as in cases of pregnancy, particularly in persons having previously born children; and provided the hypogastric tumour subsides, whilst the *os uteri* progressively relaxes, and the tumour comes within reach of the finger, it will not be unreasonable to anticipate a successful issue. The discharges which accompany these changes have the effect both of relaxing the structure of the *cervix*, and facilitating the descent of the mass, as well as lessening its bulk and firmness. Thus the body of the uterus may be enlarged by a tumour of considerable dimensions; subacute inflammation may come on from time to time; and perhaps the pressure of the mass may produce œdema of the inferior extremities. After a series of months, or perhaps years, hemorrhage takes place periodically, the *cervix uteri* gradually gives way, and at length the mass descends into the vagina, and is removed by ligature.

In distinguishing the nature of tumours much stress has been laid upon the severe kind of pain by which diseases of a malignant kind are usually characterized; but the evidence to be derived either from the absence or the presence of pain is altogether inconclusive, and it is only when a tumour is in an inflamed state that the touch is productive of pain, at least in any material degree. The mere weight of a tumour, whether malignant or not, is frequently productive of severe pain in the uterine system, especially in the upright posture; and I have several times known the attempt to apply the ligature over a large but non-malignant tumour, occasion pain of a most distressing and expulsive character, which large and repeated doses of opium would alone subdue. In the words of Lawrence, speaking of the difficulties in the diagnosis between innocent and malignant growths, " We are not sufficiently acquainted with the characteristic differences of the various accidental productions, nor with the external signs by which each of these may be recongized." Again, a tumour may be very deceptive to the eye, presenting the dark or fungoid appearance common to malignant diseases, but without possessing any such character.* Mr Langstaff informs us, that he examined the body of a woman who died from uterine hemorrhage, produced by a genuine polypus which had passed into the vagina, " and assumed the appearance of a fungoid tumour." The discharges are generally free from smell; but offensive discharge " is common (as Dr John Clarke remarks) to all diseases of the uterus in which there is a tumour capable of preventing the entire evacuation of the menstrual fluid, or of any blood which may be effused from the vessels of the uterus."

Let us now suppose the case of a tumour of a non-malignant character, entirely or in a great measure within the uterus, complicated both by its bulk and connections, its basis being extensive and not accessible to the finger, the tumour not possessing a malignant character;—what line of practice ought to be adopted?

" In practice (says Denman) it will perhaps be always justifiable to use our endeavours to extirpate every kind of excrescence of this sort." Possibly, however, the bulk may be too great to admit of an operation. Dr Hamilton admits having seen " several cases where, from the size of the polypus, an operation was deemed impracticable, and in one of these the patient lived seven years after the whole pelvis was filled with the tumour, in so much that the sufferer had been obliged during all that time to empty the bladder by means of a catheter."† Polypi and fibrous tumours are the only description of uterine tumours for the cure of which the ligature can be recommended; and no essential objection derived from their structure can be urged against the use of the ligature in the latter kind of tumour, though infinitely better adapted for the former. With this impression I should not hesitate in cases of dangerous hemorrhage to attempt its application; and in one of my last cases the tumour was thought to be broader above than below, and from its size and close connexion with the uterus it was very difficult to suspend the ligature over it. The tumour separated on the sixth day, and presented no appearance of neck.

In reference to the treatment by ligature, unquestionably the danger is considerable, and the success very problematical. The danger consists in the increased risk of including a portion of the uterus, or leaving a large portion of the tumour behind, and of the cut surface becoming the seat of bad ulceration. Should these dangers be averted, the operation may be rendered unsuccessful by the presence of other tumours, by the portion left behind not shrinking, and by successive portions descending through the uterine orifice, rendering it necessary to repeat the operation from time to time, until either the whole of the disease is removed, or the patient is no longer able to undergo the means necessary for her relief. Both these terminations have happened. A physician accoucheur, at the head of his profession, very obligingly informed me that, in one instance of the kind, the operation was repeatedly undertaken with the effect of prolonging life. The tumour, of a polypose kind, was ascertained to proceed from the whole of the interior of the uterus. In a second instance of a like nature the patient submitted several times to the operation, and was completely cured; and another similar case was stated to me, by a physician of very high eminence in this department of medicine.

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* Med. Ch. Tran. Vol. xvii. p. 65.

† Pract. Observations, Part i. p. 57, 58.

Considering all these circumstances, supposing a choice of evils presented to us, with a certain conviction that death must inevitably ensue without an operation, surely we are justified in advocating the measure, provided the patient seems to have sufficient constitutional power to endure the necessary mortification of the tumour incident to the employment of the ligature. The situation of the particular portion of the tumour intended to be strangulated, in relation to the tissues of the womb, must be attentively considered. Fibrous tumours in their early stage must necessarily be covered by the lining membrane of the womb, and surrounded by more or less of the proper parenchyma of the organ. But such portions of the mass as have descended below the uterine orifice are either destitute of lining membrane, or soon become so by a process of absorption. It seems material, therefore, to defer the application of a ligature over the isolated portion until this change has taken place. The removal of a tumour, which is entirely confined within the uterus, and adheres by a broad basis, if not impracticable, must be attended with the greatest difficulty and embarrassment, although Dupuytren has the credit of having extirpated growths of considerable bulk (polypi, I conclude,) under these very circumstances.

The question of success in all these operations will materially depend, as already stated, upon there being only a single tumour, instead of many in different stages of developement; for in the latter case the ultimate object of the operation will probably be defeated. I do not affirm that the fact of there being several tumours (assuming the point to be ascertained) is an insuperable objection to the removal of the most important of them, provided it is accessible to the finger, and the patient is losing much blood; the measure, indeed, may be necessary to her existence. Certainly, a tumour may be tied when its excision is impracticable, or where practicable, imminently dangerous ; but I cannot think with a very high authority, Dr Hamilton,[*] that " the only danger attending the ligature arises from the risk of including a portion of the uterus ;" for not only may the serous membrane take on inflammation, but the veins and arteries also. Both these terminations I have seen. In the event of severe symptoms following the application of the ligature, it is satisfactory to know that the instruments now resorted to (De Graefe's tubes especially) admit of the pressure being diminished or altogether removed with the greatest facility, and perhaps compatibly with the mortification of the tumour. Not that it is possible to determine with accuracy the length of time which may be required to arrest permanently the current of blood into a tumour, since this will depend, mainly at least, upon the bulk of the stem, and the situation of the artery or arteries, the course of which is usually, I believe, central. The principle of this is seen in one of the cases detailed in this paper.

The danger likely to arise from hemorrhage, where excision is practised, will principally depend upon the kind of connection which the tumour has with the womb.[†] If the basis is narrow, the risk of hemorrhage will scarcely equal the risk of inflammation under the employment of the ligature. I am acquainted with the particulars of several such cases, and two of these were fatal. In the first, a true polypus with a double head, the patient died previously to the ligature coming away, and extensive inflammation of the iliac veins and arteries and of the bladder also was discovered. In the second, a tumour, apparently fibrous, had been detached several days, when a rigor and other symptoms of peritonitis ensued. The changes in the serous membrane were of the usual character. The particulars of the third and fourth cases are given at length in this paper. In each of these a severe attack of *phlegmasia dolens* occurred, the result, I presume, of venous inflammation. The probability of inflammation arising and affecting such important structures, renders the practice of only partially destroying a tumour of questionable propriety, and only to be justified after mature consultation, and by a conviction that it is essential to the preservation of life. The excision of small excrescences from the *cervix uteri* is both easily and successfully accomplished, as I have repeatedly witnessed; and, as it was done by Dupuytren, it may, perhaps, be found practicable to extirpate a moderate sized pediculated growth, although altogether within the womb. But the excision of a non-pediculated tumour, under such circumstances, must ever be attended not only with great difficulty, but also with danger to life.

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[*] Practical Observations, Part i. p. 65.

[†] Dr Brown details a case of fibrous tumour attached to the left and front side of the *cervix uteri* by a broad semicircular base, in which the operation of excision was accomplished in part only. The growth was very vascular. Two arteries nearly the size of a crow-quill were observed on the cut surface. The uterine orifice was dilated, and had permitted the tumour to descend within two inches from the *os externum*. A profuse and alarming hemorrhage, which instantly followed the operation, was arrested by the plug, and the patient was ultimately dismissed from the hospital considerably relieved. She died some time afterwards under circumstances the exact nature of which Dr Brown was unable to ascertain.—Dublin Journal of Medical Science, Vol. xii. p. 360.

A friend of mine, distinguished for his skill in surgery, at one moment contemplated the removal of the whole uterus, on account of a tumour, apparently fungoid in character, which grew from the *fundus uteri*, and could not be reached by the finger, so as either to tie or extirpate it. I might mention a similar case. Both proved fatal. The formidable nature of this operation has prevented me from conforming with the urgent solicitations of two patients, both of whom were anxious for its performance.

The chief danger which attends the organic affections in question arises from the severity, the frequency, and the protracted nature of the hemorrhage. Every now and then a very favourable and unexpected change occurs in this respect, with the manifest improvement of the general health. Whether or not the case may ultimately admit of an operation, it is in the meanwhile very necessary to moderate the hemorrhage, especially during menstruation, the period of its greatest severity. For this purpose a combination of the acetate of lead combined with hyoscyamus may be resorted to; but Dr Douglas of Dublin kindly directed my attention to the increased value of the lead when combined with distilled vinegar. Should applications of a refrigerating nature prove ineffectual, a piece of lint or soft sponge may be soaked in oil, and laid gently within the vagina, not exactly on the principle of plugging, but merely with a view to favour the coagulation of the blood, and lessen the discharge. I have instructed a patient to apply this remedy herself, and have been greatly satisfied with the result.

Ice in doses of a dessert spoonful may be exhibited frequently on such occasions with good effect. In the interval of the menstrual period the muriated tincture of iron would seem to be an excellent remedy. The importance of regulating the bowels in all cases of chronic hemorrhage need scarcely be insisted upon.

Two cases of enlargement of the uterus, each terminating in the descent of a fibrous tumour in the vagina, removed by ligature.—The particulars of the first of those cases are already published.* For some years the uterus was in a state of great enlargement, and on several occasions severe inflammation of the organ took place. At length, after repeated attacks of hemorrhage, a tumour descended in the neck of the womb, which gradually dilated the orifice and ultimately passed into the vagina, and was removed by ligature. The tumour proved to be of a fibrous nature.

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* See my Work on Obstetric Medicine, p. 165.

I had not an opportunity of seeing the second case; but I am authorized to say that the tumour possessed a fibrous character, and when removed by the ligature had no appearance of a neck.

Polypus uteri treated by ligature, the ligature giving way on the fourth or fifth day, and the separation of the polypus about ten days afterwards.—Mrs —— had been the subject of *polypus uteri* some years, and was much reduced by the discharges. Her countenance was very sallow, the pulse rapid, and she was harassed with pain in the back. The polypus occupied the vagina generally, and the stem appeared to be about a third smaller than the circumference of the polypus, the size of which would not admit the finger to be passed sufficiently high to ascertain the attachments of the stem. A strong ligature of dentist's silk was applied as high upon the stem as possible. I could feel the pubic side of the *os uteri* widely open, but the sacral side of the orifice was not within reach of the finger. For two days the patient had no pain or sensible inconvenience, excepting retention of urine, evidently the result of the swollen state of the polypus. But from this time a variety of distressing sensations arose; the bladder became excessively painful on containing even a small quantity of urine, which was now opaque, and thick like gruel or purulent matter. She had great pain in the back and abdomen, vomited almost constantly, accompanied by great thirst, an increased frequency of pulse, and a most acute pain in the left knee; the slightest attempt to change the position being productive of agony. On the fourth or fifth day, the ligature on being tightened gave way, and the tubes were withdrawn. The bad symptoms immediately subsided, the polypus gradually decreased in bulk, accompanied, however, by a copious semi-purulent discharge, until at last, being above the tenth day from the giving way of the ligature, it was reduced to an inconsiderable slough, not exceeding the bulk of a moderate sized finger, when I withdrew it by a pair of forceps in the presence of my friend, Professor Partridge, and found the *os uteri* well contracted and healthy.

A large tumour, apparently a polypus, growing from the lower part of the uterus, a small portion only of the tumour having passed through the uterine orifice.—Mrs —— had suffered more or less from pain in the back for the last two or three years. Six months ago menstruation became excessive, and between the periods there has been a copious discharge of serum. The catamenial periods were also accompanied by hysteria, severe pain, loss of appetite, and incessant sickness and vomiting. I found the patient excessively pale, and complaining of violent pain in the back. The abdomen was distended from the pubes to the umbilicus, embracing two-thirds of the space laterally, by a tumour evidently the uterus, globular, even on its surface, and of a firm consistence. The *os uteri* was widely dilated, and a tumour, resembling a polypus both in its feel and appearance, had descended two inches beneath it. Between the pubes and the *os uteri*, I could pass two fingers their whole length without encountering any opposition, nothing intervening between the anterior surface of the womb and the smooth surface of the tumour, except the finger. At the sides of the uterine orifice, I was unable to pass the finger the distance of an inch within the neck, and at the posterior or sacral surface, although the uterine orifice had permitted the polypus to descend two inches below its margin, it was impracticable to pass a finger between the margin and the portion of the polypus corresponding to it. The entrance of the finger at these parts, and the further descent of the tumour, appears to be resisted by the basis of the tumour itself. On the sacral surface the uterine orifice could barely be reached with the finger. In front the orifice of the uterus, which is close under the pubes, is only two inches from the vaginal orifice, and less than two in the lateral direction, at which parts the vaginal part of the uterus is adherent to the vagina itself. The lowest part of the polypus was easily seen on slightly separating the *labia pudendi*, and a long slender polypus or appendix was seen growing from the portion of the larger one which occupies the vagina. In consequence of the largest circumference of the tumour accessible to the finger being encircled by the *os uteri*, it became impracticable to maintain for any length of time a ligature upon it, although the attempt was repeatedly made both by myself and the highly intelligent surgeon by whom she was attended. Fortunately, however, the hemorrhages have since subsided, and the general health has undergone a material improvement.

An immense polypus occupying the abdomen generally, and the vagina also, and sphacelating spontaneously.—Mrs —— aged 40, was attacked about two years ago with retention of urine, which demanded the use of the catheter. Menstruation now became excessive, and a tumour appeared over the pubes, which continued gradually increasing. At the end of two years, retention of urine again took place, and the day following I visited the patient in consultation. The uterus was greatly enlarged, occupying the abdomen generally, and extending within an inch of the ensiform cartilage. A portion of the tumour corresponding to the umbilicus was peculiarly hard, and elevated above the surrounding parts. The vagina was also occupied by a large tumour of a firm consistence, reaching nearly to the *os externum*, of a blue colour, and easily seen on separating the labia. The *os uteri* could not possibly be reached with the finger, and, owing to the size of the tumour, it was impracticable to pass the hand into the vagina. In front, the tumour was firmly impacted, and its bulk was productive of severe suffering. The constitution having become very seriously affected, chiefly by excessive hemorrhage, we were induced to recommend the application of the ligature over as large a portion of the mass as possible. At this moment, however, menstruation had just returned, and the hemorrhage being very considerable, it was thought prudent to defer the operation for a week.

On repeating our visit for this purpose we found that a very unfortunate change had taken place, namely, the sloughing of the tumour, denoted by a very copious discharge of a thick chocolate-coloured and highly offensive fluid, the apex of the tumour presenting a dark appearance, instead of the blue colour, by which it had previously been characterized. With a view of forwarding the process, a ligature was placed over a portion of the mass, but it could not be maintained in its situation. In a few days the right leg became affected with *phlegmasia dolens*; but this disease presently disappeared; the urine, previously healthy, now assumed a purulent character; the expulsive pains were most violent, and, as the pressure over the urethra and neck of the bladder was intense, we determined, with the expectation of lessening it, to excise a portion of the sphacelating mass. The quantity removed exceeded two ounces, and its excision was unattended by pain or sensible inconvenience; but we now found that the part which exerted the greatest pressure upon the bladder was too high, as well as too closely impacted to admit of removal.

The expulsive pains continued, and had the effect of forcing the tumour lower. But, notwithstanding this, an attempt now made to withdraw it through the *os externum* proved unsuccessful. The urine was discharged involuntarily, evidently from ulceration, at the spot where the pressure had been so long and severely maintained; and although the abdominal tumour continued daily to lessen in bulk, (being scarcely half the size it had previously attained,) the discharges increased in amount, and the vital powers were proportionably depressed. Death took place three weeks after the commencement of the sloughing process.

Necroscopic Examination.—The body was but little emaciated; but on opening the abdomen the bowels presented a very exsanguine appearance. The bladder was seen greatly dilated, and lying on the uterus,—the line of demarcation between them being very indistinct. The summit of the uterus extended about three or four inches above the umbilicus, and the peritoneum connecting these organs was considerably thickened; but there were no traces of lymph on the surface of the peritoneum, nor of fluid within its cavity. The peritoneum was now cut through, and the uterus with the adjacent viscera removed, but this was not accomplished without difficulty,—the uterus having contracted a very firm adhesion to the back and sides of the pelvis. Upon laying open the bladder the coats were found very much thickened, and its internal surface presented a dark-coloured and elevated appearance, its cavity containing a thick, fœtid, and purulent fluid, resembling the discharge which, during life, had escaped from the vagina. At its posterior surface, about an inch and a half above the cervix, an ulcerated opening of a circular form was discovered, which readily admitted the end of the fore-finger, and communicated with the superior and anterior part of the vagina. The walls of the vagina were thickened and very largely dilated, the mucous coat presenting an appearance very similar to the covering of the tumour which it contained. The tumour resembled a large cocoa-nut, both in size and shape. It had a dark mottled hue, and presented a mortified surface, and this more particularly at its lower part. The tumour grew from the neck of the uterus posteriorly, its basis being equal to a full-sized wrist. The finger could be passed on the anterior surface of the tumour into the cavity of the uterus, but the tumour was adherent to the posterior surface of the uterus for about two-thirds of its circumference. The neck of the uterus was everted and largely dilated, its walls being continuous with those of the vagina. The cavity of the uterus was scarcely larger than natural, neither were its walls much thickened. A small fibrous tumour projected towards the peritoneal coat of the organ. The body generally was quite healthy.

Observations.—The extraction of the mass or its greater portion through the pelvis by means of the long forceps would, I think, have been practicable some few days previous to her death, the mass having then become much softened, as well as greatly diminished in bulk. But at this time the bladder was so seriously implicated, and the constitution so severely affected as not to have justified the attempt, even supposing we could have known that the tumour, excepting its basis, was not contained within the uterus. Previously to the sloughing process having commenced, the bulk of the tumour, together with its incompressibility, would have rendered any attempt which might have been made to withdraw it through the pelvis altogether futile. Difficulties have been known to occur in the extraction of a polypus which has already separated from its attachments. "Thus, (observes Dr Hamilton,) in two different cases where the polypus was of the size of an infant's head, it became necessary to use the perforator and crotchet, and even with the assistance of those instruments, the operation proved a very tedious and a very painful one. In both instances, it required considerably above an hour before the extraction was completed.[*]

Two cases somewhat resembling the foregoing are described by Dr J. Clarke. In the first, a tumour was seated on the left side of the abdomen, the uterus being nearly as large as at the end of sixth month of pregnancy. A large tumour projected from the *os uteri* into the pelvis, and from the lower part of the tumour a process, some inches long, protruded through the *os externum*. This process was removed by ligature. It was regenerated, and the operation was repeated at different times in the course of two years. The form of the large tumour was such as prevented the application of a ligature.[†]

In the second case the uterus was greatly enlarged, and the vagina was filled with a soft spongy tumour issuing from the *os uteri*. On this tumour a ligature was placed, and as much as would fill a pint measure was brought away. It was reproduced, and the patient died. On examination *post mortem* it appeared that a soft and fibrous substance grew from about two-thirds of the internal surface of the uterus.

In Dr Denman's case [‡] the patient had already undergone two successful operations for the removal of distinct polypi from the uterus. After a time the symptoms returned, and the uterus was discovered in a greatly enlarged state. The ligature was again applied, but, as it would seem, only over that portion of the tumour which lay in the vagina, and the tumour came away on the fourth day. But the *os uteri* never closed, nor did the discharge cease.

The ligature was applied three different times at distant intervals, but without any beneficial results; the excrescence was repeatedly reproduced and descended in the vagina. The patient died under an attack of apoplexy, and on an examination of the uterus, a firm substance was found springing from its fundus and its posterior surface, and a considerable portion of the root of the adhesions were inaccessible by ligature.

A large Polypus occupying the Hypogastrium and the Vagina enclosed within the Uterus;—the os uteri adherent to it.—Mrs ——, the mother of several children, I was called to see the middle of the year 1835; and it was represented that, during the previous five years, she had been liable to uterine hemorrhages and leucorrhœal and serous discharges, and that these symptoms were associated with an enlarged state of the abdomen, the enlargement having been observed to diminish very sensibly on each attack of hemorrhage, and to increase on the hemorrhage ceasing. She had a sense of bearing down, and had experienced retention of urine.

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[*] Practical Observations, Part i. p. 70.

[†] Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, Vol. iii. p. 298.

[‡] Ibid. p. 308, see quotation, p. 317.

On examination I found the abdomen greatly enlarged, apparently by the uterus, the bulk of which corresponded with its impregnated state, about the sixth month of gestation, but with the difference of the neck being prematurely developed. When examined through the abdominal parietes, the tumour, which was insensible to the touch, was quite movable, and the integuments covering it very lax. The tumour seemed to vary in firmness in different parts, but nowhere did it possess the hardness of the subcartilaginous structure. It also appeared to be uneven and elevated, the most prominent part surrounding the umbilicus; and supposing the enlargement to consist of the uterus its long diameter, instead of being from the fundus to the orifice, must have been in the antero-posterior direction.

The vagina was occupied by a round spherical tumour, the circumference of a small orange, and of equal dimensions in every part, and had descended sufficiently to admit of its being seen on separating the *labia pudendi.* The tumour when carefully viewed appeared of a white colour, quite smooth, resembling, in fact, the exterior covering of the gravid uterus. The centre of its most depending part represented a perfectly circular aperture, the circumference of a shilling, and not unlike the *os uteri* in the first stage of a first labour. The edges were thin, apparently rather denuded of mucous membrane, and so closely in contact with a substance within the uterus, as entirely to resist the passage of the finger or even a probe.

This substance was of a deep colour like placenta; indeed, the appearance was not unlike the state of parts in a case of complete *placenta prævia*, attended by a trifling degree of dilatation. The exposed portion of this deep-coloured substance was not nearly so smooth as the surface of the part containing it, and imparted to the finger a moderately elastic feel, like placenta or fleshy polypus. Although the vaginal part of the uterus was generally extended, the apex and basis being of equal thickness, no adhesions had formed between this structure and the corresponding surface of the vagina. As it was impossible to contemplate any kind of operation, I recommended general treatment only. During the three years which have since elapsed, the returns of hemorrhage have been distant, and of the most trivial kind. The patient enjoys a good state of health, and experiences little or no inconvenience.

The engraving, Plate II. presents a view of the fibrous tumour found within the womb, described in Case I. p. 76.

ART. V.—*Cases of Puerperal Fever.* By CHARLES SIDEY, Surgeon, Edinburgh. [Read to the Medico-Chirurgical Society.]

IN watching, with great anxiety, during April last, the progress of several cases of puerperal fever which occurred in my own practice, my attention was strongly called to the fact, that, in several instances, persons who had been in attendance upon, or otherwise in communication with, the sick, became themselves afflicted with some form or other of inflammatory disease, principally, erysipelatous inflammation of the skin, the mucous membrane of the throat, and the peritoneum. In the following communication I have given the short notes which I possess of the cases in question, leaving the members of the Society to draw their own conclusions from the plain and simple facts which I shall state.

CASE. I.—Mrs C. was confined of her third child on the 16th April. Her labour was easy, although the pains for some time seemed ineffectual. For the first forty-eight hours after her delivery she had no complaint, which was more than was anticipated, from the feeling that she had experienced before delivery; and, indeed, for some years previously she had laboured under symptoms evidently denoting organic affection of the heart. But, on the second day subsequent to her confinement, a slight shivering came on, followed by great arterial action, restlessness and tenderness of the belly, as evinced by the sudden removal of the hand when applied to it, unwilling to admit pain; there were also great irritation of the bowels, slight vomiting, frontal headach, and thirst; the lochia were free, and there was some milk in the breasts; pulse 160. For the first three days of the fever she continued in much the same state, but after this slight tension of the lower belly came on, attended with violent pulsation of the heart, occasional wandering, (partaking more of a maniacal character than of that of simple delirium.) She sunk on the seventh day after her delivery, and the fifth from the commencement of the puerperal attack.

Mrs C.'s servant-maid (a person of a delicate constitution) was seized with symptoms of a bilious inflammatory fever on the fourth day of her mistress's illness, accompanied by severe abdominal symptoms, which her mother compared to labour pains; tension of the belly and oppressed breathing. There then appeared an erysipelatous inflammation of the right breast and shoulder-joint, terminating in an effusion of purulent matter in the arm-pit. This case, proceeding from bad to worse, went on for nearly two months, when it ended fatally with effusion into the chest and peritoneum.

Mrs C.'s husband, on the fourth day after her death, was seized with symptoms of smart fever and inflammatory sore throat, with a deep dusky redness all over the internal fauces, and small pustules over the uvula, great tenderness of the larynx, and great difficulty of deglutition. He was freely bled and leeched over the throat, and had constant warm poultices and gargles of warm bran water, with a few grains of the sulphate of zinc. His recovery was doubtful for some time, but he ultimately got well.

Case II.—Mrs M. was confined of her seventh child on the 19th of April. Her labour was easy, although a little tedious. During the last months of pregnancy she had been strongly impressed with the idea that her delivery was to prove fatal, and this bore heavily upon her mind. She was bled during the progress of labour, and some glysters were administered on the second day after her confinement. She had some shivering, followed by quick pulse, frontal headach, pain of the right iliac region, and tenderness upon pressure, or a sensation of being bruised; the uterus was enlarged but soft, the reverse of what is felt in common puerperal hysteritis; the lochial discharge continued, and there was milk in her breasts; her breathing was heavy. Four or five cupfuls of blood were taken from the arm, which produced fainting and abatement of the symptoms. This was followed up by hot turpentine fomentations to the belly; and with warm poultices frequently renewed, and doses of calomel and extract of hyoscyamus. On the third day the pain or uneasiness of belly returned, with vomiting of black-coloured fluid, and great tension and oppression of the breathing. A large blister was applied over the belly; the head being much affected it was freely leeched, and she took calomel and opium in repeated doses, and had besides assafœtida glysters. On the fourth and fifth the symptoms became much worse, and she appeared almost maniacal. She died on the sixth day from the attack.

Five cases of erysipelas among Mrs M.'s friends happened during the week following her death.

Mrs M.'s sister-in-law, who had visited and assisted her during her illness, was, four days after Mrs M.'s death, seized with symptoms of bilious fever, and great abdominal irritation, of which she died within eight days. Another sister-in-law, unmarried, was seized with a smart fever and erysipelatous sore throat, which continued for some time, but she ultimately recovered.

The mother-in law, who was in constant attendance, was seized with fever and erysipelatous inflammation of the face and head, followed by great debility.

A boy (Mrs M.'s son,) of five years of age, was seized with fever and erysipelatous inflammation of the face.

A girl (Mrs M.'s daughter,) of seven years of age, was seized with fever and sore throat of a dusky redness, which continued for some time.

Case III.—Mrs J. was confined of her second child on Saturday, 22d of April. Although her labour was easy, I took some blood from the arm, in consequence of the pulse being quick, and her skin hot and dry. She appeared to be doing well until the night of the second day after her confinement, when she sat up in bed, and after this a fit of shivering came on, followed by slight pain in the pelvis, and frontal headach. I saw her early in the morning, when the pain was increased, with tension and a diffused tenderness over the belly. The uterus seemed enlarged and soft. The bowels had been freely moved the day before; the pulse was 130 and sharp; no confusion of the head; the lochia were quite free. From twenty-four to thirty ounces of blood were taken from the arm, which produced fainting and abatement of the symptoms. The blood was much cupped, and of an inky colour. Hot turpentine fomentations were applied to the belly, and these were followed up by hot poultices. Calomel and Dover's powders were given in repeated doses immediately. She remained easier, until the morning of the 25th, when a diffused pain or sensation of being bruised returned, the pulse was soft, and the skin dry. A good many leeches were applied, and the bleeding encouraged with hot poultices. After this doses of calomel and opium, combined with tartar emetic, every two hours, were given on the evening of the 25th, or early on the 26th. The belly became very tense, but easily compressed and tender; the uterus enlarged, but soft; oppression of breathing, and much thirst. A large blister was applied all over the abdomen; the bowels were acted upon with glysters of assafœtida and turpentine. On the evening of the 26th, she felt easier, and had some sleep; the pulse 116, and soft. On the 27th, she had much thirst and dry skin; she felt languid, and had slight vomiting of yellow bile. Some brandy and wine was given every hour or two. During the night she had some wandering, but was sensible when spoke to. She had some twitches of the muscles of the face; her breathing was heavy; and on the morning of the 28th she expired.

Appearances after death.—In the cavity of the peritonæum there was a large quantity of serous effusion, rendered turbid by the diffusion through it of numerous flocculi of purulent-looking lymph. On several parts of the surface of the peritonæum were patches of recent yellowish coagulable lymph; in particular, a thin and imperfect coating of the substance was observed on the lower or concave surface of the liver, and along the lines of contact of some of the contiguous folds of the small intestines. The peritoneal surface of the uterus presented some patches of coagulable lymph, but none of the other tissues of this organ seemed in any degree affected. The omentum at one point was bound by an old adhesion to the *fundus uteri*. The right broad ligament and ovaries appeared very vascular. The uterine veins and absorbents seemed unaffected. In several parts the peritonæum of the small intestines presented a vascularity of a straight linear form, the lines running parallel, and not transverse, as usual, to the axis of the bowels.

Mrs J.'s infant, a healthy-looking boy, when eight days old, was seized with erysipelatous inflammation of the umbilicus, which spread over the lower half of the body. The penis and scrotum became quite gangrenous, and he died in a few days.

Dr Imlach, who had assisted at the opening of the third and fourth cases, was seized, four days thereafter, with a considerable degree of fever and erysipelatous inflammation of the internal fauces and throat, which yielded to free leeching, leaving great debility.

Case IV.—Mrs H., when at the fourth month of her pregnancy, had slight uterine pains and hemorrhage. From this period until the eighth month she was occasionally troubled with threatening of abortion, and at the end of this period uterine pains came on, and a putrid child was expelled. The uterine pains ceased, and the mouth of the womb shut, although it was evident that there was a second child in the uterus. No hemorrhage attended. This took place on the 19th, and on the 21st, she was delivered of a healthy living child. The after-birth consisted of one placenta with two cords. The portion belonging to the dead child was much diseased, but the other portion was perfectly healthy. On the 22d, she was seized with a violent shivering, followed by a smart fever; the pulse 120, firm; tenderness of the uterine region; difficulty of breathing; general tension of the belly, the convolutions of the bowels appearing quite distinct through the parietes. No pain of head, or very little; the lochia continued; and milk was secreted in the breasts. Blood was taken from the arm by a large orifice. The blood was very much buffed. Hot turpentine fomentations were applied to the belly, and constant hot poultices afterwards. In the course of the day she had two dozen of leeches applied, and took ten grains of calomel and hyoscyamus. In the evening she was much relieved. Early on the 23d the pain returned. She was bled again at her own request, but without evident relief. Leeches were again applied, and large doses of opium and calomel, as well as assafœtida and turpentine glysters, administered. Pulse varying from 108 to 120; bowels moved freely. A large blister was applied. On the 24th and 25th, she continued much the same. A mixture containing the digitalis was given freely. The breathing became much oppressed, with a fixed pain over the lower edge of the liver, which nothing could relieve. On the 26th and 27th, the belly had a very peculiar appearance, being much raised in the centre, but easily compressed. She was evidently worse, and it was deemed necessary to support the strength with small quantities of brandy, and *Tinct. opii*, and beef-tea glysters. She sunk on the 28th.

On the 29th, the body was opened. Externally the abdomen had the appearance of a large cone, the umbilicus forming the top. The measurement of the body opposite to the umbilicus was 36 inches; from the crest of the ilium over the top of the cone, 17 inches; round the base of the cone, 32 inches. On cutting open the abdominal parietes, the intestines were seen extremely distended with gas, and pushed into the cone, particularly the *caput cœcum*. The peritoneal surface presented a few small patches of recent coagulable lymph. There was a considerable quantity of thick purulent lymph collected around the site of the ovaries. Both the broad ligaments were very vascular. In the substance of the uterus, near the entrance of the Fallopian tube, was found a collection of viscid purulent or softened tubercular matter, of the size of a large hazel-nut. The substance of the uterus around the collection was vascular. There were two or three incipient fibrous tumours in the walls of the uterus. With the above exception the organs were healthy. The gall-bladder contained several calculi as large as hazel-nuts; some small ones had stuck in the duct. Particles of the phosphate of lime were dispersed through the substance of the liver. The *linea alba* was six inches broad at the umbilicus, and the abdominal parietes in that situation were so extended and thin as to allow the convolutions of the intestines being seen through them. They only consisted of the skin, fascia, and peritoneum.

The child died in a few days, with symptoms of oppression and swelling of the bowels.

Case V.—Mrs C. was seized on the second day after her confinement with symptoms of fever, and great expression of anxiety. She experienced little or no pain of the belly, except tenderness of the uterus, which, as usual, was soft and enlarged, or lying loose in the pelvis like a fagged uterus; the lochia were free; little or no milk in the breasts; great pain of forehead; short dry cough. She was bled from the arm, and had hot turpentine fomentations applied to the belly. Small doses of calomel and Dover's powder were given, glysters of assafœtida were administered, and blisters were applied to the chest. This case was exceedingly tedious, and very unpromising for a considerable time. Even after the abdominal symptoms had subsided great debility ensued; the dry cough continued, with deep-seated pain in the left side of the chest about the root of the lung, which appeared to end in an abscess that burst into the trachea and was spit up.

The fever afterwards subsided, and her health was restored by residing for some time in the country.

Mrs C.'s child, when eight days old, was seized with symptoms of fever, oppressed breathing, and a tympanitic state of the belly, with tenderness, great moaning and vomiting, which continued with increased symptoms for thirty hours, when the child died.

On opening the abdomen the day after death, the peritoneal sac was found very much distended with bloody flaky serum; the intestines were rather pale. Over one-half of the stomach, and in particular over the bladder, and about the pelvis, regular vesications appeared, and in several parts the peritoneal covering, especially below the umbilicus, was raised to the size of small grapes. The omentum had the same appearance.

These are the several cases of puerperal fever which occurred in my own practice during the spring of 1837, and there happened in the practice of others, many cases with the same result, namely, erysipelatous affection.

The following fact will show erysipelatous inflammation, attended with fever, under peculiar circumstances, giving rise to puerperal fever. An out-day patient of the Lying-in Hospital was in the habit for some days of waiting upon a relation who was ill with an erysipelatous inflammation of the knee-joint. Upon her return to the Hospital she took charge of a new born-child. Two days afterwards, the infant was seized with erysipelas of the abdomen, of which it died. On the third or fourth day of delivery, the mother was attacked with puerperal fever, and she with some others sunk under it. The Hospital was quite free from the fever previous to the appearance of erysipelas on the child.

Dr Alison, after this paper was read before the members of the Society, mentioned a case which occurred to him in the Royal Infirmary, which goes a great way to elucidate the nature of puerperal fever. A young woman, who had an erysipelatous affection of the mamma, miscarried about the third month; and very soon afterwards the inflammation left the breasts, the abdominal cavity was affected, and all the symptoms of puerperal fever came on. In the course of a few days she died, and upon opening the abdominal cavity, it was found filled with a bloody flaky serum without any adhesion of the internal parts.

That puerperal fever is a truly formidable disease, filling the mind of the practitioner with an anxiety and watchfulness to which at other times he is comparatively a stranger, every one who has seen it will admit. I conceive that it is from a collection of plain and undoubted facts, that fair and legitimate deductions regarding the appearance and pathological relations of the disease can be made. It is generally noticed that, when one case of puerperal fever happens, it is soon followed by more, either in one individual's practice, or that of others. From the foregoing cases we see what relation they bear to erysipelas. We notice puerperal fever giving rise to erysipelas under certain circumstances; and erysipelas, on the other hand, giving rise to puerperal fever, and the conclusion to be drawn is perfectly clear, reasoning upon common principles. A peculiar feature of this disease, distinguishing it from others, for which it is frequently taken, is the apparent freedom from danger, and, in many cases, the unconsciousness of any danger in the patients themselves. I think it a disease quite distinct from common acute inflammation of the peritoneum, uterus, and its appendages. Combining the symptoms, the results in practice, the state of the parts disclosed by examination after death, and the facts of the above cited cases, to which many others could be added,—with the opinion of some respectable authors, I think the inference to be drawn is, that puerperal fever is merely an erysipelatous inflammation of the peritoneal covering of the pelvic region, extending, as the disease advances, over the abdominal cavity, and the body of the uterus. The fever having a peculiar type, it is a matter of fact that, in those seasons when erysipelas manifests itself as an epidemic, we have puerperal fever, more or less, prevailing.

This may be proved from many sources. It is quite well known that, when operative surgeons complain of erysipelas following their operations, however simple, obstetric practitioners have to complain of puerperal fever. For this peculiarity the years 1825 and 1826, and the present year, have been remarkable.

It is of the highest importance that a clear line of distinction should be drawn between puerperal fever and common acute inflammation of the peritoneum and uterus. It is evident that the latter disease has been frequently taken for the former, and it is clear that, from this circumstance, arise in a great measure the different views entertained on the nature of puerperal fever,—the difference in treatment, and the great difference of success in one practitioner over his fellow, both of whom have been treating different diseases; and to show the want of unanimity among practitioners upon this disease, we have only to remember the varieties or opposite modes of treatment recommended by each, and every one, more confident than another, that his is the most successful. In the cases of puerperal fever, and common acute inflammation of the peritoneum and uterus, which I have seen in my own practice, there was, to my observation, a decided difference, both as respects the prominent symptoms of each disease during life, and the morbid appearances after death. In the acute inflammation of the peritoneum and uterus we have much pain greatly increased by pressure, and the uterus, when involved in it, is hard, firm, enlarged; the lochia cease early, and the patient is much alive to her situation. In the puerperal fever the uterus is enlarged but soft, and feels under the hand as if flaccid, without any or very little contractile power; a sensation of heat and being bruised rather than acute pain is felt, the lochia continue, the pain is sometimes so slight that the patient would hardly confess any, yet when the hand was applied she would try to get rid of it. After death we have likewise a line of distinction. In the common inflammation, we have in general a secretion of purulent matter and more or less adhesion of the internal parts. In puerperal fever a secretion of a flaky serum more or less tinted, without any or very partial adhesion of the internal parts, the intestines, peritoneum, and uterus; these have in general the appearance of being bleached, with little or no vascularity.

The immediate exciting cause of puerperal fever seems to be a peculiar state of the atmosphere; and I firmly believe it to be of a highly contagious character; to the support of which opinion I could adduce many cases, both from my own observation and that of others.

As I have seen the disease to arise, in general more or less of shivering takes place within forty-eight hours; it may be longer in some instances, according to constitutional differences; this is followed by a quick pulse, from 100 to 130 in a minute, dry skin, and thirst, pain or sensation of being bruised is felt, and tenderness in the pelvic region; the uterus appears swelled or enlarged and soft, without any or very little contractile power. It has the feeling of a jagged uterus or lying flat in the pelvis; a slight tension of the abdomen is felt at first, and a little oppression of the breathing, which after the second or third day increase very much. The lochia continue, although diminished and altered in appearance; the secretion of milk goes off about the second day of the disease; violent pain of the head is felt, in others amounting only to a degree of confusedness. In the last stages of the disease vomiting of a green or dark-coloured fluid takes place, attended by delirium, having in some cases more the appearance of simple mania.

The treatment which I followed in this intractable disease was as follows. I endeavoured to make it correspond to the individual symptoms as they presented themselves, and also took into consideration constitutional differences, and the stage of the disease when first seen, always proportioning the abstraction of blood in a general and topical manner, to the actual effect which it had upon the pulse and the pain. In some cases from sixteen to thirty ounces at one bleeding, in some cases none at all, except by leeches. Second bleedings in general were found to be productive of no good effect. This was followed or preceded with warm emollient glysters and *enemata* of turpentine combined with assafœtida; hot turpentine fomentations always with sensible relief; opium and calomel at repeated intervals, sometimes combined with emetic tartar; Dover's powder; constant hot poultices, to the belly, and blisters; and in the last stages camphorated mixture, wine, brandy, and nourishing glysters were administered. But whatever treatment was pursued, I scarcely saw any recover except one; and I am confident that a great many cases reported as recoveries have not been puerperal fever in its pure form, but common acute inflammatory cases. Some practitioners declare that they never lost a patient under puerperal fever, from their peculiar mode of treatment. It may be quite true that their patients recovered; but it is questionable if the disease was pure puerperal fever. It is not easy to steer a course in the treatment of this disease, when we notice that emetics, purgatives, turpentine, bark, stimulants, and Dover's powder have all had their advocates; and that mercury, in its various forms, especially calomel, even to the enormous extent of upwards of 500 grains in the course of two, three, or four days, has been highly commended as a remedy in some of the worst forms of the distemper.

ART. VI.—*Experiments and Observations on the Cause of the Sounds of Respiration.* By ROBERT SPITTAL, F. R. C. P., Physician Extraordinary to her Majesty in Scotland, Lecturer on Pathology and Practice of Medicine, &c.*

Two theories have been proposed in explanation of the sounds perceived during respiration on applying the ear to the human chest. The first, and until recently the only one, is that which may be inferred from the writings of Laennec rather than from anything decided he has stated on the subject. According to this theory, the different respiratory sounds comprehended under the terms tracheal, bronchial, and vesicular respiratory murmurs, result from the friction of the air on the inner surfaces of the trachea, bronchial ramifications, and air-cells during the passage of the air to and from the lungs, in the acts of inspiration and expiration; and that this view of the subject was considered sufficiently satisfactory is evident from the fact, that every writer who has recorded his opinion on this topic, at least so far as my research has gone, has, with one exception, adhered to the explanation just stated. Of those who have recorded their opinions, very few have advanced anything like experimental evidence in favour of this particular theory. M. Piorry, in his work on Percussion, details the following experiment, "Take," says he, "the lungs of a sheep bled to death, or of a person who has died of hemorrhage, non-crepitant and free from any frothy matter; inflate them; imitating as nearly as possible, either with your own breath, or by means of a pair of bellows, the respiratory movements, and the observer who shall apply the stethoscope upon the vesicular structure will hear distinctly the movements of expansion of the air-cells. He will hear the puerile respiratory murmur, if the inflation be strong, and a feeble murmur, on the contrary, if the inflation be gentle."*

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* Read at the Meeting of the British Association at Newcastle, 1838.

Now although this experiment is not advanced by M. Piorry in support of what may be termed Laennec's theory of the respiratory sounds, but to combat certain views of that author in regard to the connection between the intensity of the vesicular respiratory murmur and a supposed active dilatation of the air-cells of the lungs, still it must be regarded as one apparently calculated to give authority to such a view; and we find some years afterwards the same experiment advanced directly in support of this theory by Magendie. According to this author the respiratory sounds are caused in the following manner. "At each inspiration the air is drawn into the chest, and strikes against a multitude of small tubes, giving rise to the developement of a peculiar sound, or bruit, connected with the entry and exit of the air from the organic apparatus. This appears to me," continues M. Magendie, "the most simple and correct manner of explaining the production of the respiratory murmur; it is, in fact, nothing but the noise arising from the frottement (friction) of the air against the elastic membrane which composes the pulmonary air-cells;"† and in support of this doctrine the following experiment on the human thorax is detailed. "When the lungs," says M. Magendie, "are inflated through the upper part of the trachea, and the stethoscope is placed on any portion of the surface of the chest, I can hear the bruit of the respiratory murmur most distinctly. The sound produced by the entrance of air into the vesicles is here in fact very intense; the integuments have been removed from the anterior portion of the thorax, and the muscular layer by which it is covered is extremely thin; hence any sound developed in the interior of the cavity is conveyed through the elastic parietes to the ear, without losing any of its force or distinctness."‡

Having repeatedly performed similar experiments as well on the human lungs, as on those of the lower animals, such as the ass, sheep, and dog,—whose lungs, from possessing an analogous structure to those of man, answer sufficiently well for such observations,—I am enabled to corroborate the statements of the foregoing physiologists, that the sounds observed on the inflation of dead lungs, whose bronchial tubes and air cells are free from liquid matters, resemble very accurately those perceived during life; the tracheal, bronchial, and vesicular modifications of the respiratory murmur possessing their ordinary and distinctive peculiarities, with this difference, that, when the naked lung is the subject of auscultation, the sounds are all louder, because nearer to the ear, and deprived of the usual impediments to their free transmission, which the parietes of the thorax present.

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* Piorry, Procede Operatoire, &c. Paris, 1831, p. 88.

† Magendie's Lectures, Lancet, 1834, 1835, p. 542.

‡ Ibid. p. 797.

Knowing the ease with which air can be thrown into sonorous vibrations, we admit that these experiments appear, on the first view, to afford very strong evidence in favour of the theory of which we now speak; and we are not inclined to be out of humour with M. Magendie, who complains so much of the hasty and inaccurate conclusions of others, because he has, without further examination, come to the conclusion, that "During the act of respiration, the air is compelled to enter and alternately to evacuate the lungs; hence by a *simple and natural mode of reasoning*," continues he, "we deduce the production of sound from the friction of the particles composing the atmospheric fluid against the sides of the bronchial ramifications," and the pulmonary vesicles.*

In these few simple and apparently conclusive experiments are to be found the only evidence, of an experimental nature, in support of the present theory; but, however much they appear to favour this view of the subject, on close examination it cannot fail to be remarked, that they are quite insufficient to bear out the conclusions which have been deduced from them; and show, in a forcible manner, the fallacy of trusting to any "*mode of reasoning*," however "*simple and natural*," unless based upon careful and repeated experiment. To this loose method of arguing physiologists are in an especial manner indebted for many worm-eaten and musty, but albeit curious, tomes, which now scarcely ever see the light of day, excepting under the protecting gaze of some sorely benighted medical antiquary.

The second and only other theory of the respiratory sounds with which I am acquainted, and which it is my chief object, in this paper, to notice, is that of M. Beau of the Necker Hospital, who, after numerous experiments and examinations, arrives at the following conclusions,—"1*st*, That the different respiratory sounds, not complicated with râles, are not mechanically produced by the passage, and the friction (frottement) of the air against the walls of the bronchial ramifications where they are heard. 2*d*, That they are produced by the resounding throughout the whole column of air inspired, and expired, of a sound resulting from the striking (refoulement) of that column of air against the *velum palati* or the neighbouring parts. 3*d*, That every sound produced in the upper respiratory passages must be transmitted throughout the bronchial ramifications, with its own proper character and degree of intensity. 4*th*, That in practice, we should never conclude that the air does not enter into the lungs before having examined whether a guttural sound is produced or not. *5th*, " That we can, by stopping the guttural sound, stop the respiratory murmur, of which it is the reverberation, and thus obtain, in a state of purity and fulness, certain sounds of the lungs, of the pleura, and the heart, which are often confounded with it, and which it is impossible to appreciate unless they be so isolated." *

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* Magendie's Lectures, Lancet, 1834–35, p. 796.

It does not seem to me necessary to state here the experiments by which M. Beau has arrived at opinions so opposite to those which appeared so satisfactory and conclusive, and which had so long remained unscathed by the breath of controversy, destined seemingly to enjoy the repose which is alone inseparable from long established and severely tested truths. On the one hand, however, while they do not seem to me to prove, so clearly as might have been done, some of the conclusions which are deduced from them, on the other, they appear far from meriting the objections and animadversions which have been urged against them.

Our first object of inquiry shall be as to the existence of the sound which is said to take place in " the superior respiratory passages," in the manner described by M. Beau. In the first place, then, it is a matter of familiar observation, that a blowing, rushing sound takes place both during inspiration and expiration, synchronous with, and consequent upon, the entrance and exit of the air from the lungs. This sound every one can hear distinctly, and in his own person, without any artificial aid, especially on full, free, and quick respiration; and we perceive at once that it has its site in the upper portion of the respiratory passages. When the mouth is shut, it seems chiefly connected with the external nasal apertures; when open, we would refer it almost entirely to the mouth itself; and when the latter is widely opened, to the back part of it, to which, and the neighbouring parts, M. Beau refers it, and where perhaps it is not inaptly termed the " *guttural*" *respiratory sound.* On close examination by means of the stethoscope, this sound appears to me to be loudest, and to have its origin at the upper part of the larynx; and probably is chiefly produced in the narrowest part of this, or at the *rima glottidis*, where the air, in passing to and from the lungs, and meeting with an impediment to its free passage, is thrown into sonorous reverberation; and this is quite in accordance with all observation in regard to the rapid or forcible movements of aeriform fluids.

It does not appear necessary, however, that any such contraction should exist for the production of this sound; for if air be blown through any tube, the same species of sound is easily produced and distinctly perceived at the extremity whence the air issues, and where it is most likely produced by the vibration of the air in passing out, against the edge of the extremity of the tube, which acts here, seemingly, the part of a constriction in reference to the surrounding air; and the same thing happens if the air from the tube be made to issue into one of larger dimensions, or, which is the same thing, at any contracted point in a large tube. Again, if air be inhaled by means of any tube, a sound of a similar character is produced, and which we refer at once to the aperture by which the air first enters, and where the same species of vibration seems to take place, and probably in the same manner as that we have described as occurring on the exit of the air from the tube.

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* Beau, Recherches sur la Cause des Bruits Respiratoires, &c. Archives General. Vol. v. p. 570. Paris, 1834.

The shape of the extremity of the tube, or the aperture by which the air enters or issues, exerts a considerable influence over the degree of sound. If, for example, that used be a tube with a conical extremity, such as the ordinary stethoscope of Piorry, the sound will be perceived much louder than when a tube of equal diameter throughout be used—difference of materials likewise modifies the results.

These simple experiments appear to me to render of no avail the observations of M. Raciborski, founded upon an experiment on a rabbit, in which the trachea was cut through, and the animal allowed to respire by the artificial opening, and in which state the respiratory sounds were heard as before; for although the superior parts of the respiratory passages were cut off from the possibility of having any share in the production of the sound of which we speak, still the open extremity of the trachea may have been quite sufficient for its production, and in the manner just noticed. At all events, such an experiment is far from possessing the importance which its author would attribute to it, and not calculated to disprove, or even to affect much, if at all, the theory of M. Beau. * The existence of sounds in the superior respiratory passages was well known to Laennec; but he tells us, that "all the sounds which take place in the back part of the mouth (arrière-bouche) and the nasal fossæ have no influence on the murmur of respiration."† In support of this opinion, however, he offers no manner of proof, and how far such a view of the subject is correct, will perhaps become apparent in a more advanced portion of this communication. Magendie has likewise noticed these sounds, and does not seem to think it unlikely,—notwithstanding the opinion he has formed in regard to the cause of the respiratory sounds,—that they may exert some influence on those perceived in the human chest.‡

Having proved, then, the existence of a sound in the superior re-

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* Raciborski, p. 101. Paris, 1835.

† Laennec.

‡ Magendie, op. cit. p. 793.

spiratory passages during respiration, the next point for determination is, Does this sound resemble at all the respiratory murmurs which are perceived in the lungs and trachea during respiration? To this question we find a ready answer in the words of Laennec, who, in describing the vesicular respiratory murmur, says, "This sound may be compared to that produced by a pair of bellows, whose valve makes no noise; or still better to that emitted by a person in a deep and placid sleep, who makes now and then a profound inspiration."* In these remarks we recognize the most correct observation, showing, in the first place, the similarity which exists between the vesicular respiratory murmur, as heard by means of the ear applied to the chest, and that sound which takes place in the superior respiratory passages, and which can be easily heard at a considerable distance, and without any artificial aid, and which, doubtless, is that to which Laennec alludes, "as emitted by a person in a deep and placid sleep." *Secondly*, these observations prove, what may be considered as the next step of the inquiry, the similarity which exists between the sounds of respiration and those produced artificially by blowing air through a tube, as in the case of the bellows to which he refers, and in which instance the sound seems to take place at the nosle of the bellows, consequent upon the exit and entrance of the air during the action of the instrument; much louder, of course, during the former, as the air can only find its exit by one small aperture, whereas in the latter the air enters chiefly by the large opening of the valve, where, finding easier ingress, it is not thrown into such intense sonorous vibration; and in the experiments of Piorry and Magendie, this sound may have been conveyed throughout the lungs, and modified in the same manner as the guttural sound described by M. Beau.

In order to ascertain how far, or whether the sounds perceived in the superior respiratory passages exerted any effect, either in producing or modifying the respiratory murmurs observed in the lungs, it appeared to me that some experiments were required, in which the "guttural sounds," or sounds similar in character, might be produced, and at the same time the passage of a current of air in the lung prevented. For this purpose some experiments were performed on the lungs of the lower animals previously mentioned, and in the following manner:—The largest bronchial tube of one lung, proceeding directly from the great bifurcation towards the base of the lung, was dissected out by the fingers,—which answer better than a knife for this purpose,—and all the lateral branches and extremity carefully tied. The other lung was, in some cases, left entire, or the same bronchial tube was exposed by an incision near to the base of the lung, for the purpose of comparative ob-

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* Laennec, by Forbes.

Edinr Med. & Surg. Journ.

Plate III, p. 105 & 106, V

Dr SPITTAL ON THE CAUSE OF THE RESPIRATORY SOUNDS.

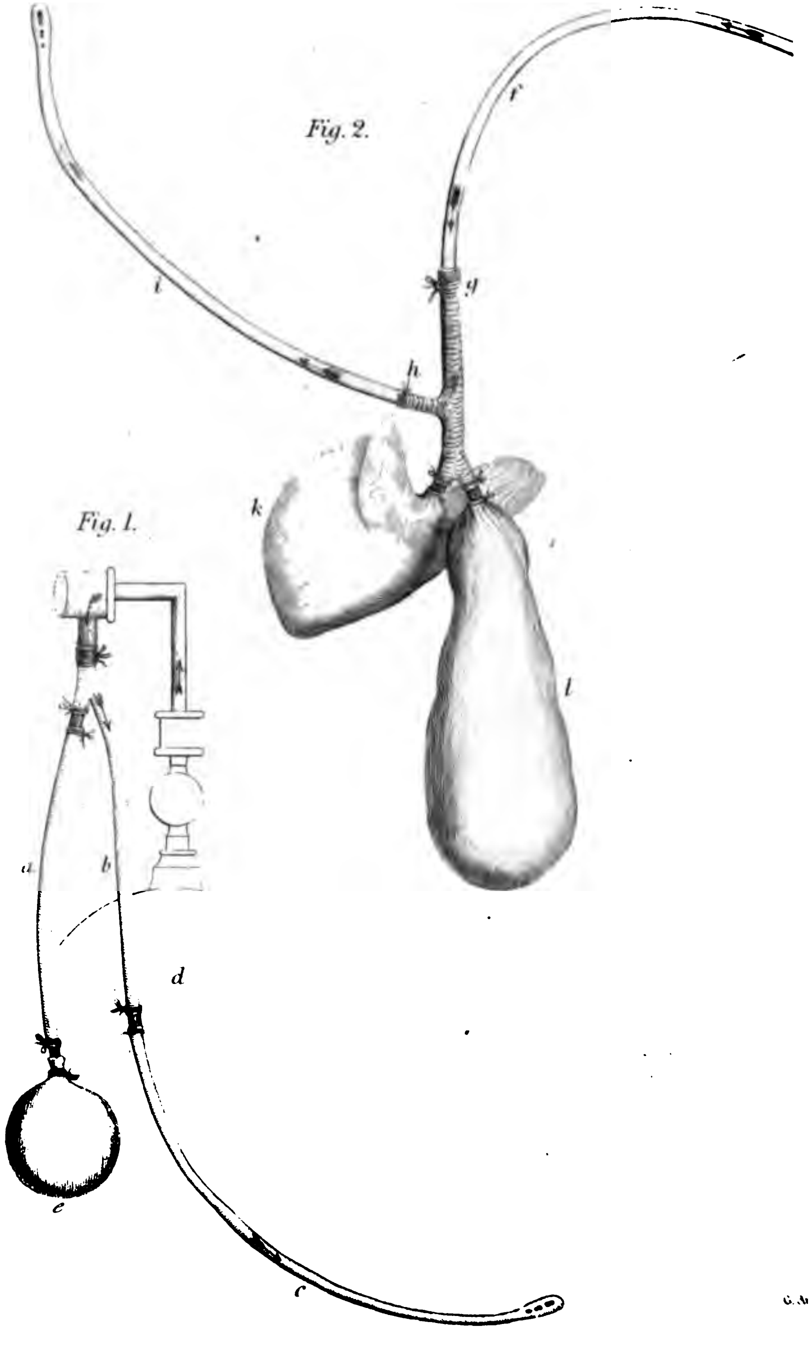
Fig. 2.

Fig. 1.

servation. The trachea was then attached to the nosle of a pair of bellows, and air blown into the lungs thus prepared. The effect of course was to distend the remaining lung and bronchial tube, while on listening, by means of the stethoscope, a loud bronchial sound was heard, almost, if not quite, as clearly in any part of the bronchial tube through which the air did *not* pass, as in that portion exposed artificially, or where it is naturally exposed before entering the vesicular tissue, and through which the air passed freely during the experiment. As, however, the stream of air from a small pair of bellows was of too short duration to admit of calm and careful observation, another method of experimenting was had recourse to, which, at the same time that it afforded ample leisure for observation, gave results more to be depended upon, and altogether more free from objections. Instead of the bellows an ordinary air condenser was employed, measuring about eight inches by nine in diameter. The interior of the cylinder of the condensing syringe used measured about an inch and a quarter in diameter, and eight inches in length. A hundred strokes of the piston were generally employed, and these afforded a continuous stream of air, lasting about five minutes, which was found quite sufficient for all the purposes of our experiments.

Exp. I. Two carotid arteries of a horse, prepared with all their lateral branches tied, were attached to the tube of the condenser, as may be seen in the accompanying diagram, Plate III. Fig. 1. The arteries were about a foot and half in length, and nearly half an inch in diameter when fully distended. The metal tube of the condenser was rather less than a quarter of an inch in its bore. One of the arteries (*a.*) was tied at its extremity, at a point immediately anterior to the attachment of the gum-bottle, seen in the diagram, so that no air could pass into it except to distend it. The other (*b*) was attached to a small gum elastic tube (*c*,) for the purpose of carrying away the air, lest the noise of its exit should have proved a source of annoyance. On the air from the condenser (*d*) being allowed to issue, both arteries became distended, and a rushing noise was distinctly perceived, which could be heard at a considerable distance with the naked ear. On close examination this sound appeared to have its site immediately at the point where the air issued from the tube of the condenser into the arteries. Its character was much the same as that produced by a small pair of bellows, but sharper, probably owing to the nature of the materials of which the whole apparatus was composed, viz. copper and brass,—varying in intensity and tone according to the quantity of air allowed to issue. This sound was distinctly perceived throughout the whole extent of both arteries on the stethoscope being applied over them ; * loudest, and of considerable intensity, at the extremities of the arteries nearest to the tube of the condenser; gradually diminishing in intensity from this point except in the artery through which the air passed, in which, when this took place with a certain force, the sound was again perceived to increase somewhat, towards the other extremity of the artery attached to the gum-elastic tube; and this appeared to arise either from the entrance of the air from the artery into the tube, which was much narrower than the former, or to be communicated from the other extremity of the same tube, where a similar sound accompanied the exit of the air. With this exception, there was not much difference in the intensity of the sound in the arteries, although it generally seemed somewhat greater in that along which the air passed, as shown in the diagram by the direction of the arrows. In order that the sound might pass freely, it was found necessary to keep the arteries fully distended with air during these observations, which was easily accomplished by regulating its exit from the condenser. Simple compression of the artery (*a*,) with the finger, anywhere between the point examined and its open extremity next the condenser, at once interrupted the sound, which was immediately to be perceived again on its removal.

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* Which was easily accomplished by means of a circular portion of cork, with a

Exp. II.—A small, thin, round, gum-elastic bottle (*e*,) was attached to the extremity of the artery, (*a*). On the air being permitted to issue from the condenser with sufficient force and volume, to distend both of the arteries, and also the gum-elastic bottle, the same sound as that described in the former experiment was heard as well in the arteries, as in the bottle; louder or feebler in the latter, according to the dimensions of that portion of it allowed to become distended. When fully distended, it measured about three inches in diameter, and in this state the sound was feebler in it than in the artery. When its dimensions were reduced to about an inch and a half, which was done by tying a portion of it while in a flaccid state,—to prevent its entire distension—the sound was much louder than in the artery to which the bottle was attached, and by which the sound was conveyed to it.

Exp. III.—In this experiment the lungs of a lamb, bled to death in the usual way, were employed, prepared in the manner seen in the diagram, Plate III. Fig. 2. An ordinary adult œsophagus tube (*f*,) of less than a quarter of an inch in its internal diameter, was attached by its rounded extremity,—on each side of which there was an oblong aperture about half an inch in length, and a quarter of an inch in breadth,—to the laryngeal extremity of the trachea, (*g*,) which measured, when distended with air, a little

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groove cut in it sufficiently large for the artery, so as to prevent pressure from the stethoscope.

more than half an inch in diameter. To the first bronchial ramification (*h*,) previous to the great bifurcation, and usually found in the lungs of these animals, and which proved so useful in this experiment, a smaller œsophagus tube (*i*,) was attached by its open metallic extremity for the purpose of carrying off the air. The rounded termination of this tube contained several apertures, all of which were closed, with the exception of a small portion of one, which could be increased or diminished at will by means of a wedge-shaped stopper. To one of the great bronchial bifurcations, a sheep's bladder (*l*,) measuring about ten inches in length, and five in breadth at the broadest part, was attached. To the other was appended one of the lungs (*k*,) which had previously been removed for another experiment; but in which the results to be presently noticed were precisely the same as in a similar experiment, frequently repeated, in which the lung was allowed to retain its natural attachments.

In this state of parts, air was allowed to enter, by means of the œsophagus tube (*f*,) from the condenser, or was blown in by the mouth. On the entrance of the air the bladder became first distended; then the lung; the air finally issuing by the smaller œsophagus tube (*i*,) when the former had become distended to a degree sufficient to counterbalance, by their elasticity, the force of the air issuing from the condenser or the mouth, in which condition the current of air must have passed along the different tubes, as marked in the diagram by the direction of the arrows. On applying the ear, by means of the stethoscope, over that portion of the trachea, where the air issues from the larger œsophagus tube (*g*,) a loud rushing sound was perceived; and the same, somewhat diminished in degree, was heard along the rest of the trachea, and also in the great bifurcation, where it was of a still more subdued but sharper tone, yet of considerable intensity. When the stethoscope was applied over the vesicular structure of the lungs, in a state of distension, a distinct rushing murmur was heard, much deeper in tone than the preceding, and at the same time more subdued; the two sounds having very much the distinctive peculiarities proper to bronchial, or tracheal, and vesicular respiratory murmurs. This sound was perceived all over the vesicular structure, excepting near any large and superficial bronchial tube, which are frequently found in such lungs, and where it was perceived somewhat bronchial. A metallic ring of about three-eighths of an inch in diameter, was now placed on the bladder, so as to divide it into two portions, for the purpose of ascertaining whether the effects of size, in the portion allowed to become distended, made any, and what differences in the character or tone of the sounds perceived in such circumstances, when it was observed, that, in a cavity measuring about one and a half or two inches in diameter, which was easily formed by pushing the ring towards the extremity of the bladder near to its attachment, there existed a loud rushing dry sound, so to speak, similar to that described as heard in the gum-elastic bottle, in a former experiment, and very analogous to a loud cavernous respiratory murmur. When smaller than this, it became decidedly sharper in tone, proving the correctness of Dr Williams' views in regard to the connection between the tone of the murmur and the size of the cavity; while, on the other hand, when larger, the sound gradually diminished in intensity, as the cavity was enlarged, until the ring was altogether withdrawn, at the same time assuming a feeble, shrill, ringing character, analogous to that form of metallic or amphoric resonance excited by the respiratory vibrations in large tubercular cavities, or in pneumothorax, in which there exists free communication between the bronchial tubes and aeriform effusion, and termed amphoric respiratory murmur, being nothing more than the cavernous respiratory murmur, modified in its character by the dimensions of the cavity in which it occurs.

These simple experiments seem to me to be less liable to objection than those of M. Beau, advanced in support of his particular view of the subject; and to render it highly probable, if not to prove, that any sound taking place in the upper portion of the respiratory passages, provided no obstacle to its free transmission exist, must, to a certain extent, pass downwards into the lungs, and more or less in every direction, like any other sound, in greater or lesser intensity, according to the nature of the conducting media.

The existence of the "guttural sound," and also the similarity between this and the sounds of respiration, appear to me to have been proved; and in order to render our experiments available, we have endeavoured to show the analogy existing between the respiratory sounds and the sounds produced artificially, as by the bellows, or otherwise. The first experiment proves that a sound of a similar character to the "guttural" can be perceived at a greater distance from its source, and in a narrow tube through which no current of air passes,—as in the artery whose extremity was tied,—than the most distant portion of the human lungs, are from the site of the guttural sound of which we speak.

The second experiment shews, that when the size of the cavity into which the sound enters is increased beyond that of the calibre of the artery, as proved by the attachment of the gum-elastic bottle, the sounds are modified according to the dimensions of this cavity; being increased beyond that in the artery, when the cavity is about an inch and a half or two inches in diameter, and diminished when extending beyond this.

The third experiment, while it corroborates the two former, presents us with other results of considerable import. In it, all the different varieties of respiratory murmur were artificially produced, or at least sounds very analogous to the vesicular, were perceived over the vesicular structure; tracheal and bronchial over the trachea and bronchial tubes; cavernous, in different tones and degrees of intensity in the bladder, contracted as described in the experiment; lastly, somewhat amphoric in this, when considerably or fully distended. While these different results were perceived, no current of air, or at least none such as in ordinary respiration, could pass through those parts, the subjects of observation; the sounds, therefore, could be nothing else than the reverberation of that sound mentioned as existing at the extremity of the larger œsophagus tube (*f*,)—and which represented here the sound perceived in the superior respiratory passages—throughout the trachea, bronchial tubes, air cells, and bladder, which latter represented very well the pleural cavity distended with air, as in pneumothorax.

From all these experiments, it seems to me reasonable to conclude, that the sound perceived in the superior respiratory passages, and which M. Beau has termed the "guttural" sound, does, and must exert a certain influence on the sounds of respiration. At the same time, we do not advance them to prove that this is the only source of the respiratory sounds, although we are strongly inclined to think so, in regard to some of them, as they are not of a character calculated to prove that the passage of the air during respiration, to and from the lungs, may not assist in producing some of them, and especially, perhaps, the vesicular; but to show that the sounds perceived in the superior respiratory passages exert, in all probability, a considerable influence, if not in producing, at least in modifying the different respiratory sounds, known by the terms vesicular, bronchial, tracheal, cavernous, and amphoric respiratory murmurs, all of which have hitherto been explained according to the theory of Laennec. Should it ever be proved, that the motion of the air in the lungs has no influence in a healthy state in the production of the sounds of respiration, we shall then be enabled to associate such an observation with those connected with the motion of liquids on solids, as of the blood on the arteries; and of solids on solids, as of the lungs on the parietes of the thorax, or the heart on the pericardium. In the former, we know, that the blood flows silently, at least, so far as our instruments can at present enable us to determine, unless some morbid contraction throw it into sonorous vibrations. In the latter, as long as the surfaces remain smooth, and sufficiently lubricated with the healthy serous effusion, the motions of the two solids on each other are performed also silently; but the slightest unusual dryness or roughness of the surfaces, at once becomes a cause of sound. So, in the case of the air, it may be found, that, except where it meets with some constriction or interruption to its free progress, analogous to that caused by the glottis, or when its force is very much increased, it passes along its course, during its ingress and egress to and from the lungs, throughout the bronchial tubes and air-cells, without producing any sound appreciable to our senses.

An additional argument in favour of M. Beau's theory, is to be found in those cases, by no means rare, in which, in consequence of a catarrhal affection in the larynx, a wheezing sound takes place in or about the glottis, and generally possessing the varying characteristics of the sibilous and sonorous râles; sometimes occurring only during inspiration or expiration, often during both. In such cases, the sounds are often heard at some distance from the patient, and are easily localized by means of examination with the stethoscope, and found to have their site exclusively in the larynx, yet having all their characteristic peculiarities conveyed throughout the whole extent of the lungs in a subdued degree.

A patient lately applied at the Royal Public Dispensary, in whose chest the following observations, bearing on this subject, were made. By means of the stethoscope applied over the lower part of the larynx and trachea, a loud tracheal respiratory murmur was heard during inspiration, and also nearly as loud during expiration; but, in the latter, accompanied with a considerable degree of wheezing of a sibilous character. At other times, a loud sonorous râle accompanied both acts of respiration. These morbid sounds were distinctly perceived throughout both lungs, of diminished intensity, but of exactly the same character as those existing at the time in the larynx, and these varied very much, occasionally possessing the sibilous, at other times assuming more of the sonorous character. When the morbid sounds in the larynx accompanied the expiration alone, similar sounds in a subdued degree were perceived throughout the lungs only during this act of respiration. When, again, they accompanied inspiration also, the sounds perceived in the lungs were easily referred to the larynx, as their source, from the fact of those of inspiration almost always differing from those resulting from expiration, and having their distinctive peculiarities conveyed throughout the lungs. The sounds were also found to increase in intensity as the stethoscope was made to approach the larynx. The vesicular respiratory murmur was generally feeble; and this seemed to be the result of emphysema of the lungs, together with a slight degree of catarrh, indicated by occasional râles, somewhat different from those of the larynx, the existence of which diseases may have exerted some influence in diminishing, in a greater degree than otherwise might have been the case, the intensity of the morbid sounds of the larynx, as perceived in the lungs, by presenting numerous obstacles to their transmission by means of the bronchial tubes and air-cells, as the feebleness of the respiratory murmur is also to be explained, according to the theory of M. Beau; the non-existence of the friction of the air to the same extent as in health, explaining the same thing, according to the views of Laennec.

ART. VII.—*Remarks by Mr John Grant Malcolmson on the Notice of his Letter on the Effects of Solitary Confinement on the Health of Soldiers in Warm Climates. In a Letter to the Editor.*

SIR,—IN your Number for April last, which I had the pleasure of seeing at Florence, a passage occurs, on which I take the earliest opportunity after my return to this country, to offer a few remarks; as it cannot fail to give rise to erroneous and very unpleasant impressions. It forms part of the very flattering notice of my pamphlet on the Effects of Solitary Confinement on the Health of Soldiers: as follows. "Part of the evils attaching to these prisons, arises, in the author's opinion, from the attendance of those establishments now falling solely on young men, owing to the late reduction of pay," &c. &c. P. 597. In this, my meaning has been mistaken, it having been my intention to express my conviction, that by reducing the allowances of the medical officers of prisons, below that of their brother officers in charge of native troops, these institutions would "*henceforth*" be placed under the care of the junior members of the service.* An old and wise regulation of the Madras government requires that no medical officer of less than three years service shall be appointed to a civil station; but it cannot be supposed that the authorities will continue to enforce an order, imposing the painful duties of visiting the sickly inmates of a great prison *with pecuniary loss*, on their more experienced servants.

So far was it from my thoughts to ascribe any blame to the medical officers of the jails of the Madras Presidency, that I believe that no institutions of the kind in any country have had abler professional attendance; and some of the prisons in which the mortality has been at different times the greatest, more under the care of personal friends, of whose qualifications for the charge it would be presumption in me to express an opinion. But I did think it due to them, when called on to notice the sickness and mortality with which they had to contend, to point out the impolicy of reducing the remuneration for such arduous duties below that of half a regiment of native soldiers, amongst whom the mortality is seldom two per cent. About that time, also, the supreme government had it in contemplation, and, as far as I know, still intend, to place these institutions in the hands of native practitioners; yet surely, public establishments in which such mortality occasionally takes place, from whatever cause, require the care of the most active, experienced, and independent men the service can furnish.

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* "It is deeply to be regretted that, by the construction put on a recent order of the Honourable Court of Directors, by the supreme government, the pay of the medical officers of civil stations has been reduced to a mere pittance; and that *henceforth* the duties of these institutions must fall on young men," &c. Note, page 5.

I am informed that the statement of the number of deaths in certain jails in Bengal and Madras, contained in the same note, has led to much angry correspondence in the Madras papers; but the same reasons that led me to publish the observations in the form of a pamphlet, for distribution in quarters where it might be of use, will deter me from entering into any discussions in the public prints. I may, however, state, that neither the prevalence of famine, nor epidemic diseases, common to the rest of the inhabitants, nor any other cause of a general nature, will account for the mortality in all the instances referred to, or for the whole effect in any one instance; and that although these are aggravated examples, selected with the view of impressing the importance of anxiously attending to the condition of prisoners generally, and never more intended, or could be fairly construed into a statement of the average mortality in Indian jails, yet abundance of other examples might be adduced from the three presidencies, that would justify the assertion in the text to which the note refers.

While I may regret the personal character these papers are said to have taken, and perhaps could wish that I had prevented misconception by a more detailed statement than I thought it proper to introduce into so short a pamphlet, I heartily rejoice that the note was written; as the discussions to which it has given rise, both in the journals and elsewhere, cannot fail to be of use, and to lead to the removal of many evils, more certainly than the most elaborate details adduced by any individual. I shall not, however, neglect the facilities offered to me while in this country, of continuing the inquiries I have been long engaged in, relative to the state of the prisoners in all parts of India; and if my health admits of it, I shall place the result of my inquiries before the public, in the form that seems to promise most advantage to the cause of science and humanity, without the least regard to any other consideration. I am satisfied that no individual experience will lead to correct results, and that it will be necessary to extend the enquiry to the three presidencies, and to compare the facts with those occurring in other countries. The worst of the Indian jails in the worst years, does not exhibit so appalling a mortality as some prisons of France and Belgium did within the present century.

That the opinions advanced in the pamphlet relative to the diet of solitary prisoners were well founded, may be inferred from the fact, that since I left India the most objectionable parts of the system have been altered. A diet of soup and butcher-meat is now allowed to all men confined for more than six weeks. The commanding officer is permitted to expend what is necessary to provide sufficient clothing for the prisoners; and those taken ill whilst in confinement and removed to hospital, have the period they may remain there reckoned as part of their service, whereas formerly they were obliged to serve the full period of sentence in actual confinement.

Forres, 20th September 1838.

Note by the Editors.

To the letter of Mr Grant Malcolmson a place has been given in this Journal, because we have pleasure in giving every one an opportunity of rectifying any misconception that may have taken place, and removing any unnecessary source of offence or irritation, especially in the discussion of questions so important as that which forms the subject of Mr Malcolmson's letter to Sir Henry Hardinge. In our account we followed as closely as possible the views given by Mr Malcolmson; and if we did not succeed in conveying a correct idea of his meaning, we trust that the present letter will remove all misconception on that head.

We think it proper, nevertheless, to state, in order that no further misunderstanding may take place,—*first*, that, whatever difference of opinion may be entertained about the cause or causes of mortality in the Indian jails, the fact of that mortality remains unquestioned; and, *secondly*, that we never for a moment imagined that this large mortality had any connection with the skill, experience, or attention of the medical officers in charge of these establishments.

The accuracy of the first statement, regarding the large mortality in these jails, is abundantly established by the facts given in the excellent Memoir of Mr Hutchinson, of which we gave an account at the same time that the letter of Mr Malcolmson was noticed. In that memoir the facts are given on official authority, and may be seen by any one who is inclined.

On the causes of so large mortality as takes place in these jails, and especially those of Bancoorah, Sherghottee, and Dinagepoor, various opinions may be entertained. But no one who knows the nature of the diseases most prevalent and fatal in these places, and who is aware of the incomparably small influence which the most vigilant and careful medical treatment exercises over them, will ever commit the mistake of supposing, that this mortality depends either on insufficient or negligent medical attendance. Fever, dysentery, and cholera may be prevented from appearing by attention to hygienic rules; but when they have once appeared, the testimony of all medical practitioners proves, that it is almost impossible to diminish the mortality which they inevitably occasion.

It is always unsafe to give opinions, and more so to assign reasons for these opinions. But the chief conclusions to which the memoir of Mr Hutchinson leads are, that the primary and essential causes of the mortality consist in the system of confinement itself, and especially in the practice of confining within places greatly too limited for the purposes of health, and often in miasmatic situations, great numbers of country labourers, who have been previously accustomed to breathe the pure fresh air of the hilly districts in India. On this head we can only refer, as we have already done, every one who entertains any doubt, to the memoir of Mr Hutchinson, and the remarks already made in our account of that memoir in our forty-ninth volume.

ART. VIII.—*Account of a Case of poisoning with Corrosive Sublimate.* By ALEXANDER WOOD, Esq. Surgeon, Edinburgh, President of the Royal Medical Society.

THE patient, a dyer in Musselburgh, named Bell, 47 years of age, was admitted, on the 3d of September, into the Royal Infirmary, where I was at that time acting under Dr Shortt. The following is his case as stated by himself.

On the 26th inst. (nine days before admission,) feeling rather unwell, he applied to an apothecary for a calomel powder, and received from his (the apothecary's) wife about a teaspoonful of a heavy white powder, half of which he took in a glass of whisky about ten o'clock in the forenoon.

While swallowing it, he felt an acute burning sensation in his throat, and immediately after, he was seized with stiffness of the jaws, and vomiting, great pain in the bowels, and the discharge of bloody stools, accompanied with cramps. Ptyalism appeared the same evening; pain in the mouth continued, with ptyalism; and occasionally the pain in the bowels was very severe.

It does not appear that at that time any medicinal means were employed likely to prove effectual in counteracting the effects of the poison. At the time of admission the patient complained more of weakness than of any actual pain, except in the jaws, which were stiff and swollen. The gums were also swollen and spongy. There was considerable ptyalism, and the breath had a strong mercurial fœtor. There was no pain in the bowels, nor was any uneasiness evinced on pressure. He had three stools, according to his own report, of tolerable consistence, and a light colour, within the last twenty-four hours. There was no symptom of any irritation in the urinary passages. The pulse was 96, rather weak, and the countenance denoting exhaustion.

Notwithstanding active treatment, the patient's strength continued to decline. Considerable quantities of fluid blood occasionally escaped from his mouth without any effort like vomiting. This was believed at the time to result from the salivation, rather than from any morbid condition of the alimentary canal. The pulse continued small, and weak, and his whole appearance very much resembled that of a person in the last stage of typhous fever. At no time, however, did he complain of the slightest pain in the bowels. He died on the evening of the 8th inst. (fourteen days after swallowing the powder,) immediately after having passed by stool about six pounds of fluid blood.

In consequence of the circumstances attending the death of this patient, a warrant was issued by the authorities for a judicial examination of the body, and Mr Watson, surgeon, was directed to perform this duty.

The following were the appearances found on dissection.*

Mouth.—The mucous membrane opposite the two last molar teeth of the right side was of a dingy green colour, and in a state of slough. Over one of the tonsils there was a small ulcer. *Pharynx.*—On the surface of the pharynx were two irregular spots, on its posterior aspect about the size of a shilling, of a brownish colour, and presenting on their surface a number of small elevations.

Œsophagus.—About three inches from its termination in the stomach, the œsophagus presented a spot similar to those described as existing in the pharynx.

The *Stomach* contained about six ounces of partly coagulated blood. On its posterior surface, and immediately below the entrance of the œsophagus, a portion of the mucous membrane was found softened, of a green colour, and part of it hanging loosely in a state of slough. The rest of the mucous membrane of the stomach was of an uniform red colour.

The *Intestines* contained throughout their whole extent, in contact with the lining membrane, a layer of dark-coloured, grumous matter, which was mixed with mucus, and in the duodenum with bile.

The mucous membrane of the duodenum was of a yellow colour, and appeared healthy, that of the jejunum and ileum was of an uniform red colour. At the termination of the cæcum, and commencement of the colon, round spots existed, less than a sixpence in size, and of a greenish colour, somewhat softened, and irregular on their surface. Near the upper part of the ascending colon a similar spot was observed On approaching the descending colon, the mucous membrane was observed to be reddened, and irregularly rough on its surface. This appearance increased on proceeding downwards, becoming very marked towards the commencement of the rectum. At some parts blood was seen effused into the submucous cellular tissue, forming little hemorrhoidal-looking excrescences. Towards the rectum the number of these diminished.

Chest.—The large veins contained some coagulated blood. The heart and valves were healthy. The lungs were spongy throughout. On making a section of the posterior part of either lung, a considerable quantity of frothy serum escaped.

Brain.—Considerable effusion had taken place into the subarachnoid cellular tissue, stretching the arachnoid membrane across the convolutions of the upper surface of the brain.

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* It is proper to state that this report is not the official one, but was drawn up when the body was publicly opened by Mr Watson, from the appearances then displayed.

The lateral ventricles contained rather more serum than natural. The substance of the brain was of its usual consistence.

Salivary Apparatus.—The parotid and submaxillary glands differed but slightly from their natural appearance, seeming only a little softer. The pancreas was natural in appearance and consistence.

Urinary Apparatus.—The kidnies contained a slight deposition of yellow granular matter, but no traces of inflammation were observable.

The bladder was quite healthy.

The patient laboured under phimosis; but no examination was made to ascertain whether or not a sore existed on the penis.

We cannot suppose, that, after the perusal of the above case, any doubt can exist as to corrosive sublimate having been the powder swallowed, so accurately do the symptoms correspond with those detailed by the best toxicologists, as resulting from the action of this powerful mineral salt.

The symptoms of irritation in the œsophagus commenced during the very act of swallowing,—a circumstance which is dwelt upon by Dr Christison as one, by which poisoning by this substance may be distinguished from that resulting from arsenic.*

This was observed particularly in the case of Mrs Hodgson, related in a former number of this Journal, who, immediately on taking the bolus in which the poison was contained, was attacked with a sense of violent burning in the throat, gullet, and stomach;† and in a case related by Mr Blacklock, whose patient "had scarcely got the drug down, when he began to retch, and to complain of constriction in his throat, accompanied with a burning sensation, and difficulty of swallowing."‡ And in a case recorded by MM. Dumonceau and Planchon, "the patient experienced the caustic effects of the poison the moment he had swallowed it." §

The bloody purging and vomiting which occurred during the first few days are also stated by Dr Christison to follow the action of corrosive sublimate more commonly than that of arsenic, "obviously," he adds, "because the former is a more powerful irritant."

But, independently of every other proof, the profuse salivation under which the unhappy patient laboured on his admission into the hospital, and the peculiar fœtor of the breath, leave no doubt that he was under the influence of some compound of mercury; while the immediate appearance of the irritant action, along with the other symptoms detailed, lead to the conclusion that corrosive sublimate was the active preparation employed.

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* Christison on Poisons, p. 35.

† Vol. xxii. 438.

‡ Edin. Med. Journal, xxxvi. 92.

§ Journal de Medecine, xlix. p. 36.

The chief peculiarities of the case, in a medical point of view, are, *first*, the early period at which salivation commenced.

It is doubted by Dr Christison whether salivation has ever occurred sooner than the beginning of the second day, and it is suggested, that, in a case described by Mr Anderson of Belfast, where it was supposed to have taken place only nineteen hours after the poison had been taken, the salivation arising from inability to swallow, owing to soreness of the throat, had been mistaken for the true mercurial ptyalism.*

In the case of Bell, however, although we have only the statement of the patient himself, that salivation occurred at tea-time, (that is seven hours after,) on the day on which the poison was swallowed, yet, after repeated interrogations, I felt convinced that in this account he was correct. That, in his case at least, there was no difficulty in swallowing, is proved by his having drunk diluents copiously, *and without any difficulty*, during the earlier stage of the disease.

The length of time during which the patient survived, while the disease was proceeding to its fatal termination, is a rare occurrence.

With regard to the ordinary duration of the disease when the patient dies of the primary symptoms, Dr Christison observes, that "it varies from twenty-four to thirty-six hours." "It is probable," he continues, "that a few may last three days; but only one instance has come under my notice where the duration was greater; and in that instance, which is described by Dr Venables, life was prolonged under great agony from pain in the belly, bloody vomiting, diarrhœa, and suppression of urine, but without salivation for eight days."†

It is true, that, in the case of Bell, the disease could not strictly be said to be in the first stage, salivation having already commenced; but we think the symptoms during life, as well as the appearances found after death, satisfactorily show that the patient died, not from any of the secondary effects of the poison, but from the consequence of the primary irritation, caused in the stomach by its use.

In the case of Bell there were no local symptoms at the time of his admission into the Hospital, or during his stay there, which could have led us to suspect the existence of the serious organic lesion which the *post mortem* examination disclosed, and, in this respect, it differed widely from the case related by Dr Venables. Though the bowels were inclined to be open, yet diarrhœa could not be said to exist, and at no time did the patient ever complain of the slightest pain in the bowels, nor betray any uneasiness when pressure was made upon the abdomen. This I consider a fact of great practical importance, from its illustrating the extent to which disorganization of the intestines may proceed without the occurrence of any *local* symptom to direct our attention to the lesion, provided always the *mucous membrane alone* be implicated.

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* Op. cit. p. 363.

† Op. cit. p. 362.

By Dr Stokes[*] it is stated, that "in acute gastritis," (as the result of swallowing acrid poison,) "there is generally great tenderness of the epigastrium, so that the slightest touch, the weight of the bed-clothes, or any muscular effort, will produce severe distress." Anxious to ascertain on what foundation this opinion rested, I have examined, as far as was in my power, the isolated cases recorded in the various medical periodicals, and I nowhere find that stress laid on these symptoms, which might naturally be expected, did they usually appear as prominently as the statement of Dr Stokes would lead us to believe. This is particularly taken notice of by Dr Mackintosh,[†] who illustrates the frequent absence of all local symptoms, where, nevertheless, serious organic lesions may be going on, by the case of a soldier of the 17th foot, who died eight or ten days after swallowing two drachms of this very substance, and in whom the stomach was found in a state of ulceration.[‡]

That ulceration of the stomach frequently does exist without any local symptoms to direct us to it, few, we think, will be disposed to deny.

In Dr Abercrombie's work on the stomach, several cases of ulceration, proving fatal by perforation, are related, where the patients had suffered only from slight and occasional dyspepsia. And the same author quotes a remarkable case, occurring in the practice of Dr Kellie, of Leith, where sudden perforation of the stomach took place, in a "strong and healthy-looking servant girl, without any previous complaint whatever." The stomach in this case exhibited extensive disease, which must have existed for a considerable period antecedently to its fatal termination.

In a well-marked case of gastritis, which lately came under my notice, I directed my attention particularly to the effect of pressure applied to the abdomen, but failed to observe that extreme sensibility which has been described. The patient had attempted to poison herself with a large quantity of laudanum, and after evacuation of the contents of the stomach, strong stimulants, chiefly, I believe, the *aqua ammoniæ*, in large and repeated doses, had been exhibited. On the third day symptoms unequivocally pathognomonic of *gastritis* appeared; but, although the general sensibility was completely restored, the patient suffered very slightly, if at all, from pressure on the abdomen. As the disease yielded to free depletion, there was no opportunity of verifying the diagnosis by actual examination; but the symptoms were such as to convince numerous practitioners who saw the case, that it was one of acute gastritis. I have dwelt thus particularly on this circumstance, because I am inclined to think that pain on pressure is very frequently absent, even in acute cases of inflammation of the gastro-intestinal mucous membrane, and, indeed, that it is rarely observed unless when the serous membrane is also implicated.*

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[*] Cyclopædia of Practical Medicine, Art. Gastritis.

[†] Practice of Physic, Vol. i. p. 297.

[‡] By the kindness of Dr Thomas Wood, I have been permitted to examine a preparation of part of the intestines of this patient, along with several drawings illustrative of the case. The large intestines were covered with numerous large black, gangrenous ulcers, similar to those described as existing in the acute dysentery of tropical climates, and a great part of the mucous membrane was hanging loose, in shreds, apparently in a state of slough.

In most of the cases which have been recorded, where any quantity of corrosive sublimate has been swallowed, the patients suffered much from irritation of the urinary passages; and to such a degree does this at times proceed, that even the external parts are found to be black and swollen. In the case of Bell, however, nothing of this kind was complained of, and the bladder after death presented none of those deviations from the natural appearance which, in similar cases, have generally been observed.

In the necroscopic appearances there is perhaps nothing more than might have been expected; but I cannot avoid drawing attention to the spots which were found at the termination of the cæcum, and commencement of the colon. They are described in the report given of the dissection, as about the size of a sixpence, of a greenish colour, somewhat softened, and irregular on their surface.

Though varying somewhat in appearance from those described as existing in the pharynx and œsophagus, yet their nature would seem to have been identical, and very similar to the one observed by Dr Venables, which, in his case, existed on the superior surface of the stomach, about midway between the cardiac and pyloric orifices, and which that gentleman describes, as "a very remarkable kind of opaque, yellowish white spot, of an irregular form, and about the size of a sixpence." I am inclined to believe that these spots resulted from softening of the intestinal mucous membrane, and that the appearances described as existing in the descending colon and rectum were produced in the same way rather than by the effusion of lymph.† "The effusion of lymph as a result either of natural inflammation, or of that caused by poisons," is stated by Dr Christison to be rare ; " at least, by no means so common as would be supposed from what is said in systematic works."†

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* In farther illustration of this, see cases recorded in Vol. xliv. of this Journal, pp. 262 and 275, and Craigie's Practice of Physic, Vol. i. p. 888.

† This peculiar softening of the mucous membrane, I think, I have occasionally observed, though in a slighter degree, on examining the intestinal mucous membrane of children who died of some disease, for the cure of which calomel had been very freely administered, and I have more than once heard *that* termed effusion of lymph, which I suspect would have shown itself, on closer examination, to have been softening of the mucous membrane.

In a medico-legal point of view, this case, though one of considerable interest, is easily disposed of. The length of time which had elapsed between the swallowing of the poison, and the death of the patient, afforded little hope of its being detected in any part of the body, and this, as far as I know, was not attempted. The question has been suggested, how far a medical man is entitled to say, that death, in any case, has resulted from the effects of poison, unless he can actually detect its presence in the body? But, with every wish to submit to the authority of those who have maintained this opinion, there are, we cannot but think, cases, where, independently altogether of the moral evidence, the witness can affirm, that the symptoms preceding, and the appearances detected after death, have resulted from the action of some powerful poison, and even, as might, I think, in this case, have been done, to speak decidedly as to what that poison was.

The statement made by the deceased, on his admission into the hospital, conveyed the impression that the powder which caused his death was given him by mistake for calomel. I believe, it turned out, on inquiry, that he had sent expressly for corrosive sublimate, having been informed, by a fellow-workman that it was an excellent cure for a form of venereal disease, under which he laboured.

This confirms an opinion expressed at the time of the *post mortem* examination, that the existence of this disease should have been mentioned in the official report drawn up, as it was extremely likely, that, had a trial taken place, this would not have proved an unimportant circumstance, especially when death was caused by a preparation of mercury.

It is impossible to be too minute in noticing the *leading features*, at least, of *every* alteration from the healthy state, which may be found in bodies, on which we are required to furnish a legal report; and in support of this opinion, the authority of Mr Watson himself may be quoted, who, in reporting two cases in this Journal, where he was directed by the authorities to examine the bodies, premises, " that the task is often difficult, and requires the utmost attention, even to very minute particulars ; for circumstances, apparently trifling in the medical evidence, often turn out to be of great importance in elucidating and connecting the moral evidence, either of the innocence or guilt of the accused." *

I cannot conclude this case without remarking, that the fact of the deceased having sent a written line to the apothecary for the drug, by no means frees that individual from blame, as it certainly would have been only a necessary precaution, before dispensing such a deadly poison, to have inquired as to its ultimate destination. Cases of poisoning from the carelessness of apothecaries are, I understand, becoming alarmingly frequent. Within a very short time two have occurred in my own experience, where, from the want of a qualified assistant, in the absence of the apothecary himself, a wrong medicine was dispensed, which, in one case proved fatal, and in the other gave rise to severe and protracted suffering.

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* Op. cit. p. 122.

† Vol. xxxvi. p. 86.

In the establishment where Bell procured the poison the individuals in charge were the wife of the apothecary and her son, a linen draper's apprentice.

ART. IX.—*Medical Report on the Practice in the Royal Infirmary of Dundee, from the 15th of June* 1836 *to the 12th June* 1837. By JAMES ARROTT, M. D., Physician to the Infirmary.

THE Infirmary was opened for the reception of patients in 1798, and during that year 6 patients were admitted. There is now accommodation for 108. The number of patients admitted this year has been 1260. The greatest number in the House at any one time has been 101, the smallest number 53, and the average for the whole year 76. The average length of time each patient resided in the Hospital was 22 days.

Since 1798, the number of patients admitted, the number of deaths, and recoveries, have been published annually ; and there was added, for the year 1835, a list of the individual diseases, and the results of the treatment in each,—undoubtedly a great improvement on the mere abstract numerical statements of former years, in as far as it enables us to ascertain what diseases were prevalent during the period to which it refers, and the rate of mortality in each. In the following Report, I shall add a few explanations, tending to afford some information, on what I conceive to be the chief objects of Hospital Reports,—viz. to ascertain the state of the public health, the increase or decrease of particular diseases, the influence of the weather on their production and modification, and the average rate of mortality in each.

The number of patients remaining in the Infirmary on the second Monday of June 1836 was 55;—and since that time there have been admitted 1205,—making a total of 1260. Dr Crichton has taken charge of the Surgical, and I of the Medical cases. The number of patients in each department, their diseases, and the result of their treatment, may be ascertained from the following Tables.

Table of 1009 *Medical Cases.*

DISEASES.		Total.	Discharged.	Died.	Remain.	Average of deaths.
Fevers, 703	*Typhus*, Typhus	700	625	50	25	1 in 14
	Hectica, Hectic	3	1	2	...	1 in 1½
Eruptive Fevers, 48	*Variola*, Small-pox	30	24	4	2	1 in 7½
	Scarlatina, Scarlet Fever	10	9	1	...	1 in 10
	Rubeola, Measles	4	3	1	...	1 in 4
	Erysipelas, Erysipelas	2	2	...	...	...
	Herpes Zoster, Shingles	2	2	...	...	...
Diseases of the skin without Fever, 8	*Prurigo Formicans*, Itching Pimply Eruption	1	1	...	...	...
	Purpura, Purples, Land Scurvy	2	1	1	...	1 in 2
	Lepra, Leprosy	1	1	...	...	...
	Porrigo scutulata, Ringworm	1	1	...	...	...
	Scabies, Itch	3	3	...	...	...
Diseases of the Head, 28	*Phrenitis*, Inflammation of the Membranes of the Brain	4	2	2	...	1 in 2
	Delirium Tremens, Drunkards' Delirium	7	6	1	...	1 in 7
	Mania, Insanity	4	4	...	...	...
	Apoplexia, Apoplexy	4	3	1	...	1 in 4
	Paralysis, Palsy	8	8	...	...	...
	Epilepsia, Epilepsy	1	1	...	...	...
Diseases of the Chest, 129	*Pleuritis*, Pleurisy	10	9	1	...	1 in 10
	Pneumonia, Inflammation of the Lungs	24	20	3	1	1 in 8
	Catarrhus et Bronchitis, Catarrh and Bronchial Inflammation	31	28	3	...	1 in 10⅓
	Catarrhus Epidemicus, Influenza	22	21	1	...	1 in 22
	Hæmoptysis, Spitting of Blood	2	2	...	...	...
	Emphysema Pulmonum, Asthma	7	5	2	...	1 in 3½
	Œdema Glottidis, Œdema of the Glottis	1	1	...	...	...
	Hydrops a Morbis Thoracis, Dropsy from Diseases of the Chest	12	9	3	...	1 in 4
	Cordis Morbi, Diseases of the Heart	2	1	1	...	1 in 2
	Phthisis, Pulmonary Consumption	18	8	9	1	1 in 2
Diseases of the Abdomen, 38	*Peritonitis*, Inflammation of the Membrane lining the Belly	2	2	...	...	...
	Gastritis, Inflammation of the Stomach	1	1	...	...	...
	Enteritis, Inflammation of the Bowels	2	2	...	...	...
	Hæmatemesis, Vomiting of Blood	1	1	...	...	...
	Diarrhœa et Dysenteria, Diarrhœa and Dysentery	22	20	2	...	1 in 11
	Cholera, Cholera	1	1	...	...	...
	Hepatis Morbi, Liver Complaints	5	3	1	1	1 in 5
	Hydrops a Morbis Ventris, Dropsy from Diseases in the Belly	4	4	...	...	...
Diseases not included in the preceding divisions, 55	*Hysteria*, Hysterical Diseases	4	4	...	...	...
	Hypochondriasis, Hypochondriacal Complaints	3	3	...	...	...
	Rheumatismus, Rheumatism	21	20	...	1	...
	Oculi Morbi, Diseases of the Eye	3	3	...	...	..
	Auris Morbi, Diseases of the Ear	2	2	...	...	...
	Anasarca, General Dropsy	8	7	...	1	...
	Chorea, St Vitus's Dance	1	...	...	1	...
	Syphilis, Venereal Disorders	11	10	...	1	...
	Cynanche Tonsillaris, Sore Throat	2	2	...	...	...
		1009	886	89	34	11$\frac{30}{89}$

Table of Surgical Cases.

Ankle-joint, Dislocation of,	1	Hip-joint, Dislocation of,	4
Disease of,	6	Disease of,	6
Bones, Ditto,	13	Knee-joint, Ditto,	6
Bladder, Ditto,	3	Lip, Cancer of,	3
Brain, Concussion of,	4	Lacerations,	33
Carbuncle,	3	Nose, Polypus of,	2
Contusions,	23	Skin, Herpetic and other affections of,	7
Dropsy,	2	Sphacelus,	1
Erysipelas,	7	Syphilis and Syphiloid Affections,	26
Eye, Affections of,	7	Shoulder-joint, Dislocation of,	4
Elbow-joint, Disease of,	7	Urine, Retention of,	4
Fracture of Extremities, Simple,	18	Incontinence of,	3
Compound,	11	Ulcers,	32
Lower-jaw,	1	Wound of Palmar Arch,	1
Clavicle,	2	Scalp,	3
Acromion Process,	1	Ankle,	1
Ribs,	1	Throat from attempt at	
Hernia Humoralis,	4	Suicide,	1

Cases which have issued in Death.

Diseased Bladder,	2
Dropsy,	1
General injury in consequence of a fall from a height of 56 feet,	1
Sphacelation from a Punctured Wound.	1

General Abstract.

Total number admitted,		251
Of whom Cured,	194	
Relieved,	20	
Irregular or improper,	10	
Died,	5	
Remain under cure,	22	
		——251

From the above statement, it appears that the number of accidents,—comprehending contusions, lacerations, fractures, dislocations, &c.—has been unprecedentedly great this last year. It is worthy of remark, however, and certainly a most gratifying subject of reflection, that, notwithstanding the multitude of mangled limbs, many of them accompanied with fracture of the bones, only *two* amputations, one at the shoulder and the other at the wrist, have been had recourse to,—thus placing the utility of the Institution in a most conspicuous and flattering point of view; as, without the facilities of treatment which a public hospital affords, very many of these poor people must have been subjected to the amputation of their mangled limbs, and so rendered mutilated objects for life.

Four cases of dislocation of the hip-joint occurring in one season is also a circumstance rather remarkable. They were all reduced in fine style, although one of them had been upwards of five weeks' standing before being sent into the House.

Dundee, June 1837. Jno. Crichton, Surgeon.

The following Table is subjoined for the purpose of comparing the results of the practice in the Dundee Infirmary with those of other places.

Hospitals.	Rate of Mortality.	Hospitals.	Rate of Mortality.
St Petersburgh,	1 in 5	Bologna,	1 in 10
Berlin,	1 . 6	Edinburgh,	1 . 10
Paris Hôtel Dieu,	1 . 7	Geneva.	1 . 11
Paris La Pitié,	1 . 8	Glasgow,	1 . 11
London St George,	1 . 9	Dundee,	1 . 13

The small average mortality in the Dundee Infirmary will no doubt be as gratifying to the Governors, as it is satisfactory to those who have had the more immediate charge of the sick. In comparing the rate of mortality in different hospitals, however, more information is undoubtedly requisite, than a bare statement of the number of patients admitted, the number of deaths, and cures. In most Infirmaries, it is the practice to strike off the books such patients as die within twenty-four hours after their admission. This rule has not, however, been followed in regard to those patients who were under my own care. Contrary also to the practice of many hospitals, we have admitted consumptive patients, which has considerably increased the rate of mortality. As we have admitted the most severe cases which could be either cured or relieved, by medical treatment, there can be no reason for concealing the fact, that a high rate of mortality may depend upon a proper selection of severe and dangerous cases, as well as upon improper management and unskilful treatment.

The following Table shows the number of patients that have been admitted into the Infirmary during the four seasons of the year, and the number of deaths during each, from 1st January 1833 to the 31st December 1836, inclusive.

	Admitted.	Died.
Spring,	912	78
Summer,	922	78
Autumn,	916	82
Winter,	934	102

The evidence afforded by the preceding table, as to the comparative healthiness of the seasons in Dundee, is in exact accordance with the evidence afforded by the Bills of Mortality: From those bills it appears, that, during the last four years, 6728 persons have been buried in Dundee, and of these the numbers in the four seasons of the year respectively have been,—

Spring,	1523
Summer,	1397
Autumn,	1781
Winter,	2207

By adding together the numbers of those who have died in the corresponding months of each of the four years referred to, we ascertain the comparative healthiness of the individual months in Dundee; and this we may compare with the healthiness of the same months in other places, by the aid of the following table; in which the months are put under the places in the order in which they are more or less unhealthy, beginning with the most unhealthy. The figures in the last column represent the number that have died in Dundee in each month for the last four years.

Rome.	Montpellier.	London.	York.	Liverpool.	Dundee.	
August	August	January	January	March	December	789
September	September	March	March	April	January	669
July	October	February	December	January	October	661
January	November	May	February	October	November	583
October	July	November	April	May	February	569
November	December	December	June	December	March	540
February	January	September	May	November	September	537
December	February	October	October	February	August	525
March	June	April	August	June	May	520
April	April	August	November	September	April	463
May	March	June	September	July	July	439
June	May	July	July	August	June	433

Celsus states that in his time spring was the most healthy season—then the winter—the summer *more dangerous*—the autumn by *far the most dangerous*. The months of March, April, and May, stand almost at the bottom of the list at Rome, at the bottom of it at Montpellier (constituting the most healthy season,) and April and May not far from it at Dundee. In London, when the average mortality was 1 in 24, the autumn was the most unhealthy season. When, on the contrary, the mortality was reduced to 1 in 40, the autumn became the most healthy; then the summer; then the winter; and, lastly, the spring the most unhealthy,—the seasons, with regard to salubrity, becoming almost the reverse of what they were at Rome in the time of Celsus, and the reverse of what they still are, in places where the mortality is great. * As these changes have not been confined to London, but have been equally perceptible in other large towns, it may be confidently expected, that, as Dundee has participated in those improvements in diet, in dress, in lodging, and in those discoveries in medical science which have contributed to the increase of health all over the kingdom; so will the mortality in Dundee be reduced to the average of other towns, when, by the opening of new streets, and the removal of all filth and moisture from their surface, by the formation of common sewers, and the disuse of all drains above ground, and by an abundant supply of water,—that great necessary of human life,—the farther conditions of their increased healthiness shall have been complied with. While, also, there is every reason to believe that the seasons themselves have undergone no alteration; and, while this change in their comparative salubrity has been invariably accompanied by a diminished mortality, it is evident that it can depend neither on the transference of the diseases of one season to another (because, in that case, the mortality instead of being diminished would remain stationary), nor upon an increase of deaths in those seasons formerly the most healthy (because, then, the mortality instead of being diminished would be increased), but must be owing to the removal from the surface of the earth, of substances which, from the influence of hot and moist seasons, give rise to those poisonous emanations which are known to have so much effect in the production of disease.

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* In Rome, 1 in 25, and in Montpellier, 1 in 31 of the population dies yearly.

The mortality in Dundee, so far as I know, far exceeds that of any other large town in Scotland. Dr Small states, in his Statistical Account, that it was 1 in 31. The population of Dundee, as ascertained by the Government census, in 1831, was 45,355. Supposing it now to be 56,000, which I believe to be rather above than under the number, this sum divided by 1923—the number of burials for last year—gives 29 and a fraction as the number of which 1 dies yearly; whereas in Manchester the mortality does not exceed 1 in 45; in Liverpool it is 1 in 44; in Glasgow 1 in 45; and in Scotland, generally, the average mortality is about 1 in 56.

Another subject, concerning which hospital reports are expected to furnish information, is the increase or decrease of particular diseases. Although we have no very accurate records of the diseases of former times, there is sufficient evidence to show that we are altogether free from many of the most deadly from which our forefathers suffered; while the severity of others is so much mitigated as scarcely to affect the mortality.

From the plague, Dundee has not suffered since 1607. Ague, formerly so frequent and so destructive to human life in Dundee and the neighbouring Carse, has now entirely disappeared.

Scurvy, properly so called, is now unknown in this town; although the name is not unfrequently applied to many other diseases such as the *lepra vulgaris*, common leprosy—(not the leprosy of the Jews described in Leviticus, but a much milder disease)—to the various species of *acne* (the common pimpled face), to the varieties of psoriasis (the dry or scaly tetter); and in consequence of the misapplied names of scorbutic, and scurvy, these and many other diseases of the skin, are not unfrequently aggravated by the use of crude herbs and vegetable acids, which our great navigator, Cook, discovered to be so very beneficial in the true scurvy.

It is to be hoped that we have witnessed the beginning and the end of epidemic cholera. It commenced in Dundee in the end of April 1832; and, during that year, the number of persons seized with it was 808, and the number of deaths, 512. It terminated at the end of December 1833; but no exact record was kept of the number of persons seized during that year. However, as the number of persons who died of cholera in 1833 is known to have been 137, on the supposition of the deaths being in the same proportion to the seizures as in 1832, the number of persons seized must have been 216,—making the total number of cases 1024, and the total number of deaths 649.

It is much to be lamented, that, although it is now forty-one years since the discovery of vaccination, by the illustrious Jenner, as effectually and permanently securing from the danger, and almost universally from the contagion of small-pox, yet no fewer than 30 patients ill of it have been admitted into the Infirmary since June 1836, and not fewer than 50, as appears from the mortality bill, have died of this loathsome disease during the past year.

Having kept notes of the state of the weather, and endeavoured to ascertain its effects in the production or modification of the diseases of the past year, I was desirous of laying the results before you; but find that they would far exceed the limits of this report. They all tend, however, to prove that great vicissitudes of the weather, but particularly great cold and wet, are among the chief sources of human maladies. Indeed, it is astonishing that opinions so contrary to fact, as the bracing effects of cold, and the danger of open winters, could ever have been received. While the strong and vigorous feel that alertness and buoyancy which a moderate degree of dry cold produces, they forget its direful effects upon the more weak and tender who are unable to resist its chilling effects. The influence of the severity of the seasons upon human life, during the period included in this report, is amply confirmed by an increase of 250 burials during the last year; and by the fact, as ascertained from the Town Chamberlain's register, that while the number of burials from June 1835 to June 1836 (the period which constitutes the Infirmary year), was 1773, from June 1836 to June 1837 it was not fewer than 2266—being an increase of 493 over the corresponding period of the preceding year, 470 having been buried in the month of January. How much more severe the weather was during the latter, than during the former period, is known to every one,—affording ample proof of the erroneousness of the popular belief in the bracing effects of cold. A comparison of the London Bills of Mortality for the years 1794 and 1795 gives a decided proof of the same fact,—the winter of the one being the mildest, and of the other, the most severe on record; and the deaths during the severe winter being almost double those of the mild. The practical inference from all this is plain, that we ought to secure ourselves against the pernicious effects of cold, by abundance of warm clothing; and that, although the chief object of the Infirmary is to heal the sick, we ought not to forget, that prevention is better than cure.

As statistical information, although it may gratify the curiosity, can never either become satisfactory, or useful, unless it indicate the causes of the facts of which it treats, I shall add a few remarks explanatory of the tabular view of the medical cases.

I. *Fevers.*—The total number of persons affected with typhus fever, as appears from the Table, is 700 ; of these, 342 were males and 358 females, showing that fever is nearly equally prevalent in both sexes. But the mortality is considerably greater among males, than among females, for, of 50, the total number of deaths, 31 occurred among males and only 19 among females, which, omitting the fractions, is equal to 1 in 11 among the former and only 1 in 18 among the latter.

The following Table shows the age at which fever is most common, and the great increase of mortality as life advances.

Age.	Number of cases.	Number of deaths.	Average of deaths.
Under 10,	32	1	1 in 32
10 and under 20,	194	4	1 48½
20 do. 30,	305	14	1 21 11-14ths
30 do. 40,	111	12	1 9¼
40 do. 50,	36	10	1 3 3-5ths.
50 do. 60,	16	7	1 2 2-7ths.
60 do. 70,	4	1	1 4
70 do. 80,	1	1	1 1
80 do. 90,	1	0	0 0
Total,	700	50	1 in 14

The same Table divided into periods of 20 years.

Under 20,	226	5	1 in 45 1-5th.
20 and under 40,	416	26	1 16
40 do. 60,	52	17	1 3 1-17th.
60 do. 80,	5	2	1 2½
80 do. 100,	1	0	0 0
Total,	700	50	1 in 14

The two preceding tables show distinctly that the danger from fever increases as age advances. They do not afford so decided information as to the age at which fever is most common ; because we do not exactly know how many people there are of each age, and we must be careful not to build our knowledge on the quicksands of false arithmetic,—a fruitful source of error, not only in medicine, but in every other science where quantity and frequency are concerned.

The day of the disease on which the patients were admitted has been extremely various, from the 1st to the 21st, and had no inconsiderable effect in determining the result of the cases. Thus, the average day of the disease on which the patients who recovered were admitted, was the 8th ; while the average day of disease on which the cases that terminated fatally were admitted, was the 11th.

The day of the disease on which the 50 cases that terminated fatally were admitted was as follows :

Number of cases that terminated fatally.			
1	Admitted on the	3d	day of the disease.
3	-	5th	-
1	-	6th	-
7	-	7th	-
4	-	8th	-
9	-	9th	-
3	-	10th	-
4	-	11th	-
1	-	12th	-
3	-	14th	-
6	-	15th	-
1	-	21st	-
8	Unknown.		
Total, 50 (51)[*]			

Of the 8 in regard to whom the day of the disease is unknown, the probability is that they were all admitted at a very advanced period of the disease; as on admission, some of them were delirious, and could give no account of themselves; the rest were moribund.

Bearing in mind, that, out of the 700 cases, only 12 died who were admitted during the first week of the disease, the Governors of the Infirmary, and the public, have the strongest inducement to recommend application for admission immediately on being seized; as, by that means, not only is there a greater chance of a favourable termination, but also the risk from the spreading of contagion is very much diminished.

We thus see how the danger is increased with advancing years, and with the advanced state of the disease on admission. The circumstance, however, that, of all others, has appeared to me most to increase the fatal tendency, is the previous frequent indulgence in the use of ardent spirits and other intoxicating liquors; but, as most of those who are addicted to the unhappy vice of drunkenness, particularly females, endeavour to conceal this debasing passion, it is impossible to state in numbers or reduce to calculation its baneful effects.

Any lengthened discussion as to the nature or treatment of the present epidemic fever, would, in this report, be entirely misplaced. The fever of the past year, however, has been distinguished by the frequency with which it has been complicated with inflammatory affections of the lungs. As it is not uncommon for diseases to be influenced by the prevailing epidemic, perhaps this may have been owing to influenza having prevailed so much during the last Infirmary year. However this may be, when the influenza, which commenced in May 1836, began to decline towards the end of July, fewer of the cases of fever were complicated with disease of the lungs. Affections of the head, however, became proportionally more frequent; but these again gave place to affections of the lungs, when, towards the end of the year, influenza appeared with a frequency and severity hitherto unknown. In the months of October and December affections of the bowels were not unfrequent among the fever cases; a few patients were altogether free from local complication,—that is to say, no symptoms of local disease in any particular part of the body, predominated over the general symptoms of fever.

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[*] There is some error here; but we merely mention the fact that the summation amounts to 51.

It is quite obvious from these facts, that any one mode of practice, or the adoption of any single remedy in fever, must not only be unavailing, but often extremely injurious. To be a *routinist*, would be to be unsuccessful,—it is only by minute, patient, and continued investigation, carefully distinguishing the peculiarities of each case, and treating it accordingly, that a satisfactory result is to be expected. The chief difficulty during the past year has been rightly to balance between the dangers of local inflammation, and the depressing and debilitating influence of the disease on the general system.

II. and III. *Diseases of the Skin*.—It does not appear necessary to make any farther remarks on the diseases of the skin than what have already been made in a previous part of this report.

IV. *Diseases of the Head*.—Although we have to remark the absence of many diseases of the head from our list, it is lamentable to have to record the increase of *delirium tremens*,—the delirium of drunkards,—a disease only produced by over-indulgence in the use of intoxicating liquors.

V. *Diseases of the Chest*.—The cases of disease of the chest amount to 129,—nearly one-seventh of the whole medical cases. This great number is undoubtedly owing to the prevalence of the severe and changeable weather, throughout almost the whole period to which this report refers. Persons of all ages suffered from these diseases, but they were particularly severe upon the old and infirm; indeed, an unusual number of people of advanced age have been cut off by the late inclement season, and there are few families that have not had to regret the loss of more or fewer relatives within the last twelve months.

Few of these diseases were altogether free from complication,—many of the cases combining, in various degrees, the symptoms and characters of pneumonia, catarrh, asthma, and consumption. In the tabular view, I have arranged, under each head, those cases in which the disease under the name of which it is placed existed without complication, and those in which it predominated; their nearly contiguous place in the table gives an idea of their connexion and relative frequency.

VI. *Diseases of the Abdomen*.—The diseases of the abdomen, as might have been expected, from the coolness of the summer months, are comparatively few in number, particularly those disorders which are more peculiarly the product of the hot season, such as cholera and bilious vomiting; and even many of the cases of diarrhœa and dysentery occurred in December, and were accompanied by catarrhal and rheumatic symptoms, implying an origin distinct from the bilious diarrhœa and bilious vomiting of summer.

VII. *Diseases not included in the preceding Divisions.*—The only other disease which I think it necessary to mention, is rheumatism, of which there were a great many cases, generally so severe as to confine the patients to bed. The frequent co-existence of this complaint with catarrh and dysentery scarcely requires to be noticed.

The classification of diseases which I have adopted, I am aware, is liable to not a few objections, (for what artificial arrangement of natural objects is free from imperfections?) I trust, however, that the governors of the Infirmary are more desirous to obtain that information which it is the object of this report to give them, than to raise objections against it, or quarrel with it for its simplicity; and they are aware that it is written for the public, and not for the profession. Should any of the governors observe in it a few names of diseases which they have not been accustomed to hear, they need not on that account be under any apprehension that the catalogue of human maladies is on the increase. They may assure themselves, that in proportion as affections of particular parts of the body have been discovered and named, the asthmas, the difficulties of breathing, the nervous diseases, the putrid fevers, and the dropsies,—which, to speak correctly, are not so much diseases as symptoms of other diseases,—have disappeared in a fair proportion; and the disappearance of these is, in fact, the best evidence of the advancement of the medical art.

The only subject not strictly medical, to which I think it necessary to allude in this report, is the office of chaplain,—an office, for the existence of which, in our Infirmary, the governors are chiefly indebted to the Reverend Mr Jaffray and Mr Edward Baxter, who, along with a number of other gentleman, defray the expenses of this appointment. The primary object of the Infirmary being, undoubtedly, the restoration to health of the patients admitted, many were afraid that, however well intentioned those persons might be who recommended the appointment of a chaplain to it, yet, such an appointment might in some degree defeat the objects of the institution. A regard for truth, however, requires me to state, that, in so far as I know, not only has no such result followed, but, on the contrary, while Mr Wallace's ministrations have been extremely acceptable and useful to the patients, they have in no instance been productive of mischief.

In conclusion, I must express my thanks to my colleague Dr Crichton, for his kind advice and able assistance. I have also to express my obligations to Mr Allan, the late apothecary, not only for the accuracy with which he carried my directions into effect; but also, for that valuable assistance which a practical experience of five and twenty years in the superintendence of this Infirmary, and in the management of the sick, enabled him to give me.

I return the Directors and Governors my sincere thanks for having placed me in the honourable, although responsible situation, I hold in the Infirmary. I trust that this report is, in some degree, evidence that I have not neglected the important duties committed to my care, and that the information contained in it, particularly in regard to our epidemic fever, may tend to promote the objects of the beneficent institution under their charge.

JAMES ARROTT, M.D.

3, *St Andrew's Street.*

		Highest temperature.	Lowest temperature.	Av. temp. for month.	Days of frost.	Days of rain.	Days of snow.	Prevalent winds.	Patients admitted into Infirmary.	Burials.
1836	June,	74	47	59.04	0	7	0	W. & S.	104	121
	July,	71	50	56.01	0	9	0	W.	88	95
	Aug.	74	51	59.77	0	3	0	W. & S.	104	148
	Sept.	65	40	53.14	0	10	0	N. & E.	91	149
	Oct.	62	28	47.20	4	7	2	N. & W.	87	178
	Nov.	52	32	42.25	1	10	0	W.	91	161
	Dec.	52	27	38.87	7	6	4	W. & N.	114	287
1837	Jan.	47	25	37.43	8	5	6	W.	118	470
	Feb.	55	29	40.76	1	9	0	W.	95	202
	March,	52	24	40.45	7	4	4	W. & N.	110	173
	April,	55	31	41.91	3	8	4	N. & E.	95	136
	May,	68	38	47.78	0	5	0	N. & E.	94	146

Since writing the preceding report, it occurred to me that the above table, showing the state of the weather, the number of patients admitted into the Infirmary, and the number of burials in the town, in each month during the period to which the report refers, might, with propriety, be substituted for the more ample details, which their length prevented being inserted in the body of the report. For much assistance in constructing this table, I am indebted to the kindness of Lieutenant Smart in permitting me to inspect the accurate Meteorological Table kept by him at Earl Grey's Dock: and to Mr W. B. Baxter, the Town's Chamberlain, who kindly permitted me to examine the official register of burials kept by him.

J. A.

PART II.

CRITICAL ANALYSIS.

Art. I.—*Des Maladies, considerées sous les Rapports Medical, Hygienique et Medico-Legal, &c. Mental Diseases, considered in a Medical, Hygienic, and Medico-Legal point of view.* By E. Esquirol, Chief Physician to the Royal Lunatic Asylum of Charenton, &c. &c. 2 Vols. 8vo. With an Atlas of 27 Plates. Paris, 1838. Pp. xviii. and 1532.

The volumes now before us do not contain a systematic treatise on Insanity, but a collection of papers which M. Esquirol has, from time to time, inserted in the *Dictionnaire des Sciences Medicales* and medical journals, and which he has now revised, modified, and enlarged. The order, however, in which they are arranged is such as to give a sort of continuity to the whole information contained in them; and they now appear with nearly as much advantage as if they had been thrown into a more systematic form. Repetitions occur no doubt, but, being of important facts and doctrines, they are perhaps useful.

The papers are divided into three classes. The first contains memoirs on Insanity, Hallucinations, Illusions, Fury, Mental Alienation occurring in puerperal women and nurses, Epilepsy, the Critical Terminations of Insanity, Lypemania or the melancholia of the ancients, Demonomania and Suicide. These are followed by others on Monomania, Mania, Dementia, and Idiocy. The second class contains Statistical and Hygienic papers; and the third, a few devoted to Medico-legal questions connected with Insanity. In the present article and its continuation we shall examine the memoirs of the first and third class only, and notice the second in a subsequent paper, which we shall devote to the consideration of some other works on the Statistics of Insanity and the Management of Lunatic Asylums.

I. Insanity, *(Folie.)*—After a striking though somewhat declamatory introduction, the greater part of which has been frequently quoted by writers and lecturers, M. Esquirol takes a general view of the symptoms, causes, progress, terminations, and treatment of insanity.

1. *Symptoms.*—M. Esquirol defines mental alienation to be a cerebral affection, *usually* chronic, unattended with fever, and characterized by disordered states of the sensibility, intellect, and will.

The sensibility of the insane is increased or perverted; their sensations have no longer any connection with external or internal impressions; their errors of sense and their illusions completely influence their actions. Many do not read, because the letters appear to cross each other (*chevaucher*,) and thus prevent them from forming them into syllables or words. Delirium is produced and fostered by numerous delusions of sight. Relations and friends are mistaken for strangers or enemies, and the properties of surrounding objects are very imperfectly appreciated. Food is very often obstinately rejected with disgust after smelling it; and not unfrequently, at the commencement of the disease, the taste is perverted, and all nourishment refused. This last symptom speedily disappears when the state of the stomach, on which it depends, is removed. Many form very inaccurate judgments of the weight, form, and volume of the substances which they touch; and most of the insane become inexpert at manual occupations, awkward, and incapable of rectifying the errors of the other senses by touch. Though when the disease manifests itself, and sometimes for a long time previously, smell and taste are altered. But errors of hearing and sight most frequently characterize and keep up the delirium of the greater part of the insane. Some hear voices speaking most distinctly, and carry on with them coherent conversations. These voices come from clouds and trees, penetrate through walls and pavements, and follow and harass, night and day, in society and in solitude, the unfortunate patients who hear them. They assume the accent and tone of a relation, friend, neighbour, or enemy.

Insanity also is characterized and continued by a multiplicity of sensations, by an exuberance of ideas, and a versatility of affections, which occur in a confused and disorderly manner, without an object, and without permanence. This exuberance of thoughts prevents the patient from fixing his attention on each sensation and idea long enough to enable him to separate those which have no connection with each other, and discard those which are superfluous. Hence he can no longer ascertain the qualities and connection of objects, compare or form abstract ideas. The result of this is a fugacious delirium, the object of which incessantly changes, and assumes new shapes. His language and actions participate in this inconstancy and versatility; and the thoughts are sometimes elevated, and even sublime. In otehr circumstances, again, the attention is energetically and exclusively devoted to one object, from which nothing can distract it, and all reasoning and determinations flow from the fixed idea. Of this monomania presents numerous instances.

The faculty which the mind possesses of associating sensations and ideas, of combining them with each other, and with the determinations, is altered in a very remarkable manner in the insane. The slightest impression, or the most feeble consonance, produces strange associations. Of this M. Esquirol gives some striking instances, of which we select one as a specimen. The town of Die is commanded by a rock, which is called the V. A young man took it into his head to add the letter v to the word Die, and thus transformed it into Dieu. All the inhabitants of Die became gods *(Dieux)* in his estimation. Discovering, however, soon the absurdity of this system of polytheism, he concentrated the divinity in the person of his father, as being the most respectable person in the place, and his father, though two hundred leagues distant, acted in him, and he only by his father, &c.

The organs of other insane patients, from their debility, receive sensations feebly, and, not being sufficiently sensible to impressions, their memory does not retain them, and they recollect only those circumstances which are long past. Ill assisted by their sensations and memory, they can neither form any notion of the relations of objects, nor fix their attention. Their determinations are uncertain, and they seem to act only from reminiscence.

In some cases, the individual, in a manner withdrawn from the government of the will, appears to be no longer master of his actions, and is forced to commit acts which he himself condemns. Such impulses, irresistible directions, or automatic determinations, as some authors call them, though they appear to be independent of the will, may often be referred to motives of which both the patient and those who watch him can give an explanation. The insane, as Locke has observed, are like those who reason correctly from false principles and come to erroneous conclusions.

The symptoms which characterize insanity produced by moral affections impress on it all the traits of the passions. Determinations, however, which are thus produced by the passions have no resemblance to the way in which the individual was affected at other times, or to that observable in other persons. A madman is enraged, or jealous, and he commits murder. He is impatient at being detained, and, if he cannot escape, he throws himself out of the window or sets fire to the house.

Some are struck with terror, conceive themselves to be ruined, or the victims of some conspiracy, or dread death. Others believe themselves happy, are very gay, think only of the good which they enjoy, and the benefits which they can scatter; are persuaded that they are elevated to the highest dignities; that they are entitled to the homage of the whole world; that they dwell in a higher region; and that they ought to live eternally intoxicated with delight, &c. This happy state, in which some madmen pass their time, has given rise to many erroneous opinions in respect to the insane. Those who have seen such cases have rashly concluded, that madmen are all happy, and that they have no suffering, while the reverse is generally the case; the insane undergoing as much misery physically as they do mentally.

The passions of the insane are impetuous, especially when they are affected with mania and monomania, sad in lypemania, while in dementia and imbecility those only are exhibited which arise from the first wants of man,—love, anger, jealousy.

It has been said that fury is but a prolonged fit of anger. In like manner, erotomania may be considered to be love carried to excess, religious lypemania to be zeal or religious fear pushed beyond bounds, and suicide to be a fit of despair, &c. Thus from the calmest state a man may verge, by imperceptible degrees, into the most violent passion, the most furious mania, or the deepest melancholy, for all forms of insanity have their primitive type in some passion.

The moral character of the insane undergoes great changes. Men of the strictest probity and most irreproachable conduct, act in a way diametrically opposite to that which might have been expected from their previous life. Their pusillanimity is very remarkable. They are timid, jealous, and suspicious. Hence they are comfortable in no situation, wish to be anywhere except where they are, and distrust and detach themselves from their relations and friends. Distrust characterizes nations whose intellect is not developed. The least suspicious and the most confiding individuals are those who cultivate their intellect; so true it is, that moral power is proportioned to the developement of the intellectual faculties. Despite, however, their distrust, the insane are as improvident as savages. Regardless of the future, though extremely anxious about the present, they are exposed to privations of every kind, unless they be watched and carefully attended to. It has long been observed, that the insane entertain an aversion to individuals dear to them, and injure, maltreat, and avoid them. This is the natural consequence of their distrust, suspicion, and fear. To this rule, however, there are exceptions. Some preserve a sort of affection for their relations and friends, which, though sometimes excessive, is unattended with confidence in those, who, before the disease came on, had influenced their ideas and actions. The melancholic patient adores his wife, but is deaf to her advice and entreaties. The son would sacrifice his life for his father, but will show no deference to his counsels when they have any reference to his disease. This moral alienation is so constant that M. Esquirol considers it to be an essential character of insanity. There are insane patients whose delirium is scarcely perceptible. But there are none whose passions or moral affections are not disordered, perverted, or destroyed. The restoration of the moral affections to their natural condition, the desire again to see their children and friends, the tears of sensibility, the anxiety to open their hearts, to be again in the midst of their family, and to resume their habits, are certain signs of recovery. The contrary denotes approaching insanity, or a threatened relapse. The abatement of delirium is not a certain sign of cure, unless the affections of the patient return to a natural state.

Such, according to M. Esquirol, is a general view of the moral and intellectual symptoms of insanity; and though some objections might be made to many of his statements, and he does not display that metaphysical acuteness which is to be found in those German authors who have written on the subject, we willingly admit that, for all practical purposes, the description is accurate enough, and, therefore, instead of discussing the debateable points, proceed to lay before our readers an abstract of the physical symptoms.

In some insane patients, the vital powers are increased to such a degree, that they resist those influences which are most calculated to injure the health. Some, for instance, harassed by internal heat, will divest themselves of all clothing, and throw themselves into water in the most inclement seasons, while in others the muscular power acquires an energy which is formidable, on account of the force which it gives their fool-hardiness, and their delirium rendering them incapable of appreciating danger. Many neither eat nor drink for days, and yet preserve their muscular vigour. These instances, however, are rare, instead of being common, as is generally supposed. Most insane patients crowd round the fire when the weather is at all cold, and almost all eat much and frequently. Scurvy does not spare the patients in those hospitals in circumstances which are favourable to its developement, and epidemic and contagious diseases find their way into asylums. Such instances prove that madmen are not so unsusceptible of external influences as has been maintained.

The features of the insane have a convulsed appearance, and are much altered from their natural state. Some individuals are plethoric, others lymphatic, some strong, others weak, and the pulse varies remarkably. Their agitation and melancholy is increased after meals, and they have acrid nidorous eructations. Some feel a sort of languor of the stomach, which prompts them to take stimulants, while others have pains in the abdomen, and burning sensations in the intestines. Maniacal, monomaniacal, and lypemaniacal patients in general do not sleep, and this state of wakefulness continues for months. Should sleep overtake them, they are harassed with nightmare and fearful dreams, and awake with a start. Those affected with imbecility and *dementia* would always sleep. Some are constipated for many days, and the urine is occasionally retained for twenty-four, sixty, or even one hundred and twenty hours. In other cases the fæces and urine are passed involuntarily. All their excretions have a penetrating odour, which saturates clothes and furniture, and which nothing can destroy. Many are affected with excruciating headaches, inducing them to strike their heads, or with pains in the chest, abdomen, or limbs, which they attribute to their enemies, to the devil, or to ill usage. They are liable also to cutaneous affections, wounds, hæmorrhoids, convulsions, and organic diseases.

From the statements now made, it may be concluded, that in the insane the vital properties are altered; that moral and physical sensibility, the power of thinking, comparing, and associating ideas, will and memory, the moral affections and the functions of organic life, are more or less injured.

After recovery has taken place, the insane, according to M. Esquirol, have the most perfect recollections of their true or false sensations, their reasonings and consequent determinations, and the nearer the cure is of being completed, the more powerfully do they remember the smallest details. Therefore, during delirium, they possessed judgment (*connaissance*) and reason. That such is the case, in some instances, cannot be doubted. But we protest against this being taken as a general rule, and maintain that the conclusion drawn from it requires to be considerably modified. In whatever meaning M. Esquirol uses the word *connaissance*, whether it be perception, conception, or judgment,—and he is by no means accurate in his language,—it is manifest that, strictly speaking, it is inapplicable to the insane; for though in many cases it may be true that they reason accurately from false premises, it is impossible that those patients whose ideas rush in torrents through their minds, as in furious mania, can reason in the slightest degree, seeing that they cannot detain their thoughts long enough to compare them with each other. This indeed is admitted in what follows.

All the disorders of the understanding are referred by M. Esquirol to the state of the attention. In the maniac, the impressions are so numerous and fugitive, and the ideas so abundant, that he cannot fix his attention sufficiently on each object or idea; in monomania, it is so concentrated on one subject that it is not directed to surrounding objects or accessary ideas; while in dementia, the organs are too feeble to assist the attention, and there is neither sensation nor understanding. By one of these three causes the attention of the insane is impaired. When the attention can be roused by any powerful impression, the individual becomes sane so long as the effect remains, in other words, so long as he can direct and keep up his attention. Imbecile and idiotic patients are altogether destitute of this faculty, and thus are incapable of education.

M. Esquirol divides insanity into five genera; 1*st*, *Lypemania* (the melancholy of the ancients,) in which the delirium is limited to one or a small number of subjects, with predominance of a sad or depressing passion; *2d*, ***Monomania***, in which the delirium is limited to one or a small number of subjects, with excitement and predominance of a lively and expansive passion; *3d*, ***Mania***, in which delirium extends to all subjects, and is accompanied with excitement; *4th*, ***Dementia***, in which the insane rave in consequence of the organs of thought having lost the energy necessary for the performance of their functions; *5th*, *Imbecility* or idiocy, in which the organs have never been so well formed as to enable the individual to reason correctly. Insanity may assume successively and alternately these different forms. They may all alternate or be accompanied with each other in the same affection and in the same individual. M. Esquirol conceives monomania to be more frequent than mania, and dementia and idiocy to be more rare than the latter.

2d, *Causes*.—Though climate appears to have little effect, the seasons seem to exert no little influence in the production or aggravation of insanity. It was remarked by the ancients that summer and autumn produced fury; and M. Esquirol has adduced several facts to show that excessive heat or cold induces insanity in those unaccustomed to them. Both heat and cold affect the insane, the former producing excitement, the latter depression. Great atmospherical commotions also excite and exasperate them, and hence they are more obstreperous, and require more care at the period of the exquinoxes than at any other time. From a table of the admissions at Salpetriere for nine years, it appears that the number was greater during May, June, July, and August, and gradually decreased from September to March. The course of insanity, too, appears to be influenced by the seasons. Some individuals pass the summer in depression or excitement, and the winter in an opposite state. If mania, which appears in spring or summer, be acute in its course, and if it be not rapidly cured, it terminates in winter. The monomania or mania of autumn does not terminate before spring. Summer is most favourable for the cure of dementia. Recoveries which take place in the hot season of the year are more rare, but more permanent. Relapses are apt to occur at the season in which they first made their appearance, and are more frequent in spring and summer, than in winter. In some forms of intermittent insanity relapses occur with perfect regularity at the same season, even after an interval of several years.

Many authors affirm that insanity is epidemic, and M. Esquirol asserts that, in some years, independently of moral causes, it spreads to a great number of individuals. Insanity very rarely occurs in children, unless from congenital malformation or convulsions which induce imbecility or idiocy. M. Esquirol, however, mentions four cases, three of them in his own practice. In youth, mania and monomania appear in all varieties and shades, while lypemania belongs to middle age, and dementia to old age. In youth the progress of insanity is acute, and the crises in which it terminates are more apparent. In adults it is more chronic and complicated with abdominal affections, cerebral hemorrhage, and palsy; it terminates more slowly by hemorrhoids and alvine evacuations, and recovery is more uncertain. Dementia, however, may appear in youth, and mania and melancholy in advanced age. In reference, then, to age, insanity may be thus distributed: Imbecility occurs in infancy, mania and monomania in youth, lypemania or melancholy in manhood, and dementia in old age.

From various statistical observations M. Esquirol concludes, that insanity in men occurs most frequently between 30 and 40, and in women between 50 and 60; that in both sexes fewest cases occur in childhood, youth, and old age; that in women insanity appears at an earlier age than in men; and that from 29 to 30, more among the rich are attacked with insanity in proportion to the number furnished by them, than among the poor.

The statement of Cœlius Aurelianus, that women are less liable to insanity than men, still holds good in Italy and Greece. In the north of France, on the contrary, the reverse is the case. In England the numbers are more nearly equal. M. Esquirol finds, in the education and occupations of his countrywomen, circumstances sufficient to account for the discrepancy. From various facts cited by him, he concludes, 1*st*, That among a numerous body of the insane, the difference between men and women is less considerable than has been supposed; 2*d*, That this difference is nearly that of the difference in the proportion of the sexes in the population; 3*d*, That the difference is not the same in all countries; and, 4*th*, That in France the proportion of insane women is greater than in England.

In women there are causes of insanity peculiar to their sex. Physical causes act more frequently on them than on men, more of them become insane before twenty years of age, they are more subject to dementia, and their delirium is either erotic or religious. While the disease lasts, they are more reserved than men, speak with greater reluctance of their condition, and try to conceal it from themselves and others. Men, on the contrary, are more maniacal, more furious, and more confident in their delirium, which is frequently complicated with hypochondriasis. The success in treating them is greater, and they are not so subject to relapses as women.

In practice a simple temperament is so rarely met with, that it is not easy to state with precision that of any individual. The sanguine temperament predisposes to mania, the nervous temperament to mania and monomania. Individuals of a dry temperament, in whom the abdominal organs predominate, are timid, restless, and predisposed to lypemania. The lymphatic temperament is to be met with in mania and monomania, in which cases dementia is to be dreaded. The temperament of imbeciles and idiots is not characteristic. Generally speaking, those individuals who have black hair, and who are of a strong, robust, and sanguine temperament, are maniacal and furious. Those who have fair hair, blue eyes, and a lymphatic temperament, become maniacal or monomanical, and their insanity becomes chronic, and passes into dementia. Those of a dry nervous temperament, who have black hair and dark eyes, are most frequently affected with lypemania. Those whose hair is of a fiery-red, are furious, treacherous, and dangerous.

Professions which expose men to the vicissitudes of fortune, predispose them to insanity, and a sedentary life, whether from choice or necessity, acts in the same way. The same may be said of occupations in which individuals are exposed to carbonaceous or metallic vapours. Habits of intoxication and debauchery of every kind lead to the same results. The abuse of stimulants and of venery are not, however, always causes. In many cases they are merely symptoms of the disease.

According to Humboldt, there are few insane persons among the savages of America; and Carr states, that they are rarely met with in Russia except in large towns. In France they are less numerous in the rural districts than in cities, and the disease is more apt to assume the erotic or religious form. The prevalence of insanity in England is attributed by M. Esquirol to ill-assorted marriages, especially between individuals predisposed to the disease, to hazardous speculations, to indolence in the rich, and to the abuse of stimulants. In Italy, in consequence of the manners of the inhabitants, religious melancholy, and erotomania, are very frequent. The ignorance of the middle ages produced demonomania and vampirism, which are still met with in the uncivilized districts of the north of Europe. A great proportion of the insanity found in France is justly attributed by M. Esquirol to political agitations, laxity of morals and improper education. Respecting, however, the long agitated question, whether insanity has increased in that country since the Revolution, he comes to the conclusion, that the increase is more apparent than real; that it is not so great as has been asserted; and that it is owing less to the storms incident to that mighty change, the influence of which was temporary, than to the alteration of manners, the effects of which are more permanent.

Of all the moral causes of insanity, pride, timidity, terror, ambition, reverses of fortune, and domestic vexations, are the most frequent. Had the last embraced only a single idea, M. Esquirol would have placed it before the others, as it is by far the most common, especially among women. Gay affections of the mind are rarely the cause of insanity; and it may be strongly suspected where any of these is assigned, that some other circumstance which is not apparent has produced the disease. In illustration of this he cites two instances, one in which an individual became mad on being appointed to an important situation, and another in which the same result attended the gaining a prize in the lottery. In both of these cases excessive joy was supposed to be the cause of the disease, whereas, in reality, the former became insane from despair at quitting his mistress, and the latter from the fear of being plundered.

The struggle between religious principles, morality, education, and the passions, if long continued, ends in insanity, and characterizes some forms of lypemania. According to M. Esquirol, however, religious fanaticism has lost its influence in France in the present day, and is very rarely a cause of the disease. In eight only of 600 patients, could this be assigned as the cause of the disease: and of 337 admitted into his own establishment, one alone was produced by ascetic exaggeration.

In France the passion of love which, in warm countries so often produces erotomania and nymphomania, seems to have lost its power. There it is neither exalted nor pure enough to engender erotic insanity.

A succession or complication of moral causes may produce insanity in cases where the mind is perfectly capable of resisting the influence of one of these; and moral causes, especially in women, are frequently combined with physical. The moral causes are more common than the physical, and the proportion of the former to the latter is greater among the rich. The latter act more frequently on women than on men, as might be expected from the disorders attending menstruation, pregnancy, and nursing, especially in the inferior ranks. The forms of insanity usually produced by them is dementia.

As there are certain conditions of the atmosphere in which epidemic or contagious diseases become more frequent, so there are certain general states of mind in which insanity is propagated to a number of individuals by a sort of moral contagion, of which several instances are on record.

The general causes of insanity may be termed hygienic, intellectual and moral. The special may be named physical. Such are hereditary predisposition, convulsions in the mother during gestation, epilepsy, disordered menstruation, parturition, &c.

The most common predisposing cause of insanity is hereditary constitution, and the predisposition is more apt to be inherited from the mother than from the father. The predisposition is less when the children are born before the developement of the disease in the parent, and greater when both parents have the same tendency. According to Burton, the children of aged parents are liable to melancholy. But this we suspect is a mere fanciful hypothesis. Hereditary predisposition to insanity is detected in the physiognomy, the form, the ideas, habits, and propensities of those destined to become its victims. The disease thus inherited appears at the same age, is produced by the same causes, and assumes the same character in the children as in the father.

Circumstances occurring during gestation, nursing, and teething are often the cause of various forms of insanity in the child. Violent impressions made on the mind in early childhood may also induce the disease. Blows on the head, severe fevers, and disordered menstruation, are well known causes of insanity. Continence very rarely produces insanity. M. Esquirol has seen some girls who lost their senses after being ravished, and has treated two females who became insane on their marriage. A sixth of the insanity of women in his experience was produced by disordered menstruation. Derangement of the catamenial discharge does not invariably accompany insanity, though at the menstrual periods the disease is always aggravated. Insanity appears more frequently after parturition, and during nursing than in pregnancy.

Suppression of the cutaneous exhalation, the presence of mucous or bilious matter in the stomach, and worms in the intestines, produce insanity.

Epilepsy sooner or later tends to the same result. Of 300 epileptic patients at Salpetriere, more than a half are insane. The same is observed at Bicêtre and Charenton. Some who suffer from epilepsy are affected with idiocy or imbecility, others with dementia, and some are maniacal and furious. The fury of epileptic patients is most ferocious.

Hysteria and hypochondriasis frequently degenerate into insanity, of which they are sometimes the first stage; a circumstance which has led many authors to class them under mental alienation. Apoplexy frequently terminates in dementia complicated with paralysis. Insanity has been produced by suppression of the discharge from the nose, of gonorrhœa, the drying up of an ulcer or issue, and the repelling of cutaneous affections, gout, and rheumatism. But though these generally precede the affection, they are not always its cause. The abuse or even the use of medicines which act powerfully on the nervous system, and various poisons, in addition to their primary effects, produce a form of insanity which is difficult to cure. Asphyxia from charcoal gives rise to incurable dementia.

3. *Course and terminations of Insanity.*—The causes of insanity do not always act on the brain, but on organs more or less distant. Like other diseases it has its period of incubation, and in the accounts given of patients by relatives, circumstances are mentioned which show that the disease had been going on for a considerable period before it was suspected. The effect of the disease is sometimes mistaken for the cause, and frequently the habits, tastes, and passions of the individual undergo a change long before he becomes actually insane. For instance, speculations of a hazardous nature are entered into. They fail, and the disease is erroneously attributed to the failure, when the speculation itself was the effect of insanity. Another is seized with a fit of deep devotion, goes to church, hears a sermon which terrifies him, and comes home with the impression that he is damned. In this case the sermon could not have produced this effect, if the disease had not previously existed.

Insanity is continued, remittent, or intermittent. Continued insanity pursues a regular course for a definite period, and has three well marked stages. The first is acute, and accompanied with concomitant symptoms, the second chronic, and free from all symptoms unconnected with delirium, and the third is the period of decline and cure. This course, however, is clearly observed only in the acute or accidental forms of the disease, or in the paroxysms of intermittent insanity. The remittent form exhibits some remarkable anomalies both in the character and duration of the remission. In some cases it is but the passage of one form of delirium into another, as when a patient passes three months in lypemania, the next three in mania, and four months or more in dementia, and thus successively, sometimes in a regular manner, sometimes with great variations. In other circumstances, again, the remission consists of a sensible diminution of the symptoms of the same form of insanity. Thus, certain maniacal patients become agitated and violent only at certain hours of the day, on certain days, or at certain seasons, while their delirium is of a calm and peaceable description at other times. The same may be said of lypemania.

Intermittent insanity may be quotidian, tertian, quartan, monthly, annual, or the fits may return after several years. This intermittent variety is sometimes regular, at other times irregular. In the former case the same seasons, the same period of the year, or the same physical and moral causes, reproduce the affection with the same characters, the same crises, and the same duration. More frequently, however, the attacks recur at very uncertain intervals, are produced by new causes, and assume a different form of delirium. Their duration and crises are different, and the paroxysm, though sometimes occurring suddenly, is more frequently ushered in by various symptoms, which are usually the same as those which preceded the first attack.

It has already been observed, that insanity undergoes a sort of transformation, and that different forms of delirium replace and succeed each other. They do more, however; they are complicated in such a manner as to form binary and ternary compounds. Lypemania is complicated with mania, and dementia with mania and monomania. Insanity is frequently complicated with cerebral affections, such as chronic inflammation of the meninges, paralysis, &c. or with affections of the lungs, heart, intestines or skin; and patients affected with mental alienation, not being exempt from inter-current and epidemic diseases, are influenced by them in a more or less marked degree, whether the progress of the disease be suspended or put an end to, or the days of the patient cut short.

M. Esquirol believes that the doctrine of crises is as applicable to insanity as to other diseases, that the cure is certain where the affection terminates in a marked crisis, and that when it terminates suddenly, without any such obvious cause, it is much to be feared that it is assuming an intermittent type. The reason why insanity passes so frequently into the chronic states, he conceives to be, that the critical efforts are rarely perfect, and frequently abortive. This is caused by the weakness and susceptibility of the patient, and by causes of a debilitating nature, and the irregularity of the symptoms disturbing the course of nature.

The ancient authors, as well as Boerhaave, Pinel, and other physicians, have noticed the critical terminations of insanity. These, however, whether moral or physical, occur only in monomania, lypemania, mania, and acute dementia, and are never met with in imbecility, chronic or senile dementia.

When insanity terminates in resolution, the alteration in the colour of the face, which before was bright red or earthy brown, the calmness of the features, the feeling of general lassitude, relaxation of the skin, freedom of the excretions, and the return of moral sensibility, indicate convalescence, which becomes perfect when reason is restored, the usual evacuations re-established, and the patient resumes his usual habits and character. If, however, the sleep, appetite, and excretions become natural, and neither the delirium is diminished, nor moral sensibility manifested in the same proportion, monomania or mania passes into a chronic state, or degenerates into dementia.

Insanity sometimes terminates in the absorbent system predominating, the patient becoming stout, and the delirium declining in proportion as the obesity increases. Obesity continues for several months after a perfect cure. But if delirium continues it is a symptom of dementia. In some, on the contrary, recovery takes place only after extreme emaciation.

Galen records an instance of insanity terminating in a quartan fever; and Balgarric mentions a similar case. Hippocrates, Celsus, Zacutus, and Boerhaave state that it ends in a hæmorrhoidal discharge; and Frederic Hoffman advises cupping-glasses to be applied to the *anus* for the purpose of inducing this. Epistaxis and menstruation also prove critical. Coition, the excretion of the semen, and pregnancy have been mentioned as crises; and hence marriage has sometimes been recommended. M. Esquirol very properly cautions his readers against this, as it has not succeeded so often as has been imagined, and has sometimes increased the evil. In his own experience he has met with many monomaniacal and maniacal patients in whom the disease has resisted pregnancy, parturition, and nursing.

Cutaneous affections deserve attention, both because their suppression induces mental alienation, and because the insane are very liable to them. The influence of the progress of the pellagra on suicide will be afterwards noticed. In the meantime the following facts may be mentioned. Insanity appears sometimes simultaneously with tetters, and in general does not terminate till the eruption disappears. Recovery is permanent only when the impetiginous affection fixes itself in one point.

Hippocrates thinks that itch puts an end to the disease; and all who have had much practice in insanity have had it in their power to verify this opinion. M. Esquirol, on one occasion, attempted to give the disease to a military man affected with dementia and paralysis, in consequence of the eruption being repelled, but succeeded neither in curing the patient nor producing the affection. Gardanne contends that insanity may be cured by inoculation with the small-pox. Furunculus, which produces suppuration, often puts an end to mental alienation, while eschars and extensive but debilitating suppuration never produce a favourable result. The re-establishment of ulcers, the drying up of which has induced insanity, promotes recovery. Swelling of the parotid or salivary glands and salivation occasionally prove critical. A flow of tears may also be looked upon in the same light. The return of the cutaneous exhalation is more frequently a cause of cure than has been supposed; and it is in this way that the season of spring and warm-baths act so favourably. Among the other critical terminations of insanity, M. Esquirol enumerates the vomiting of mucous, yellow, black, or pitchy matter, alvine evacuations of the same nature, and the expulsion of worms. One species of mental alienation may prove critical to another; and all forms of the disease terminate in dementia after a period of longer or shorter duration. Some patients after being cured become hypochondriacal and hysterical. Epilepsy never puts an end to insanity. Convulsions resembling epilepsy occasionally occur; but these, far from being critical, are symptoms of cerebral disease, and mark the approach of death.

Moral affections are, in the opinion of Esquirol, critical, whether the disease depends on disorder of the brain or any other organ.

The conclusions of M. Esquirol respecting the curability of insanity are, that the number of absolute cures is about a third;* that the number of cures varies from a quarter to a half, according to the peculiar circumstances of locality, disease, and treatment; that the cures are more numerous in France than in England. On this last conclusion M. Esquirol felicitates himself not a little. We suspect, however, that it would not be found to be well-grounded, if we had accurate returns from all our asylums, public and private.

M. Esquirol further states, that the greatest number of cures takes place during the first two years; that the average duration of curable cases is from a few months to a year; that after the third year the probability of cure is reduced to a thirtieth; that the more favourable age is from 20 to 30, and that beyond 50 recovery is rare; that many more cases of mania and monomania are cured than of lypemania; that neither idiocy nor senile dementia are ever cured; that chronic dementia is rarely cured; and that patients affected with mania recover more rapidly than those affected with lypemania. All these conclusions are corroborated by those most actively engaged in the treatment of the insane in this country, and are agreeable to what might have been *a priori* suspected.

Some patients, susceptible of cure to a certain extent, are incapable of mixing with the world, in consequence of their great liability to relapse, and hence require to be kept in retirement. Others, though they become rational enough, have received such a shock as to render them incapable of attending to business of any kind. The proportion of these is about a twentieth of the whole number of cures.

Most insane patients retain a disagreeable feeling of their disease, and are often ungrateful for the care bestowed on them, because they conceive that their malady has been mistaken, and that they have been improperly treated. This feeling, which is very marked at the commencement of their convalescence, disappears as their health is restored. Almost all patients, even those who have been maniacs, recollect the various illusions, hallucinations, &c. with which they were affected during their illness, and many in consequence dare not appear in public, from the fear that they may be objects of curiosity, commiseration, or suspicion. It happens not rarely that individuals regarded as cured by their relations and medical attendant are in reality not entirely sane. They reason perfectly well, resume their former habits and style of living, and even perform important duties, while there is something singular, eccentric, and unusual in them. This, however, disappears after a time, unless a fresh attack be induced.

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* At Ivry it was 263 in 529, or 1 in 2.01.

About a tenth of those cured relapse. Among the rich the proportion is less, because they have it more in their power to avoid those causes which produce a relapse, while the poor remain exposed to all their influence. M. Esquirol protests against attacks being confounded with relapses. Physicians are aware that those who have been affected with inflammation or other diseases become liable to a fresh attack. The same is the case with insanity. He does not deny that the insane are liable to relapses; but these can always be foreseen and frequently prevented. This distinction, however correct it may be abstractedly considered, appears to us to be of little practical importance, since it is of little consequence whether such cases be denominated fresh attacks or relapses. Greding, Monro, and Crichton, state that the insane are not long-lived even when they recover. M. Esquirol believes this to be true to a certain extent, though it is by no means rare to find patients who have been twenty, thirty, and forty years in an asylum.

The mortality of the insane depends on many circumstances of a local nature. It is greatest in autumn and winter. Taking both sexes together, it is greatest from the thirtieth to the fortieth year, in women from the fortieth to the fiftieth, and in men from the thirtieth to the fortieth. It is greater in women than in men after the sixtieth year. Hence death occurs more early in men and in a greater proportion in women in advanced life. At Marseilles, Raymond fixed the mortality at 1 in 14. At Paris, Leven, in 1786, made it 1 to 11, and Pinel, deducting cases of senile dementia, 1 to 20, and 1 to 23. M. Esquirol believes the proportion to be 1 to 6 or 8. In mania, he states it to be 1 in 25; in monomania, 1 to 16; in lypemania, 1 in 12; and in dementia, 1 in 3. Imbeciles and idiots, though incurable, sometimes live long, though they do not commonly survive their thirtieth or fortieth year. Accidental and acute mania is rarely dangerous. Simple lypemania, even when marked by a tendency to suicide, is dangerous only when it depends on organic disease, or is complicated with scorbutus, phthisis, or paralysis, in which case the patient becomes affected with marasmus, and dies. Dementia, being the termination of all forms of insanity, is most commonly fatal.

The mortality of the insane is greater in the first two years of the disease than afterwards, and among the women at Salpetriere, greater the first year after their admission.

The diseases which most frequently put an end to the life of the insane are meningeal inflammation, cerebral fever, apoplexy, organic lesions of the brain, thorax, or abdomen. Deducting epilepsy and paralysis, the proportions are two-eighths of encephalic affections; two-eighths of diseases of the thorax; three-eighths of diseases of the abdomen, comprehending under these colliquative diarrhœa and marasmus occurring without organic affections. This last is contrary to the opinions of Greding and Munro, who found the chest most frequently affected. It is, however, drawn from the inspection of about 600 subjects. This, we are of opinion, may depend on climate.

Slow nervous fever frequently terminates the career of those affected with lypemania. The patients refuse to move, some decline all nourishment, others eat with voracity; in fact, they seem to take a delight in encountering everything which can destroy their organization. They waste, and become extremely feeble, their skin becomes of an earthy colour, paroxysms of fever come on every evening, and diarrhœa frequently supervenes and hastens their death.

The complication of phthisis with insanity was observed by Mead and Lorry. M. Esquirol has several times seen it precede lypemania, and mania by several months, or appear simultaneously with them. It may escape the most attentive observer. The patients grow weak, marasmus and slow fever, sometimes cough and diarrhœa, ensue, and the delirium, far from diminishing, increases to the end. Sometimes insanity alternates with phthisis, and in such cases all the symptoms of consumption disappear during the continuance of even the most violent delirium, and return after the paroxysms.

Scurvy is one of the most frequent complications of insanity. It is often the consequence of the disease of unhealthy habitations, want of exercise and bad regimen. Those so attacked are affected with lypemania, dementia, and very frequently with paralysis.

Most of the insane, particularly monomaniacal patients, whose cases terminate fatally are paralytic. The old are more liable to apoplexy than the young.

Respecting the pathology of insanity, M. Esquirol comes to the following conclusions.

1st. Malformation of the cranium is found only in imbeciles, idiots, and cretins.

2d. Organic lesions of the encephalon and its coverings are observed only in those cases in which the disease has been complicated with palsy, convulsions, or epilepsy, or else these lesions are attributable to the disease of which the patient died.

3d. Sanguineous or serous effusions, injections or infiltrations met with in the cavity of the cranium, thickening and adhesion of the meninges to each other, to the cranium, or to the cineritious substance of the brain, partial or general softening of the brain, thickening of its substance, and tumours of various kinds found in the cranium, indicate the cause or effects of insanity, or else the complication of this with the disease of which the patient died.

4th. The alterations observed in the thorax, abdomen, or pelvis are evidently independent of insanity. They may sometimes, nevertheless, indicate the origin of the disease, by pointing out the organ primarily affected, which reacted on the brain.

5th. All the organic lesions observable in the insane are found in individuals who have never been affected with chronic delirium.

6th. In the bodies of many insane patients, no alteration is observable, though the disease may have lasted several years.

7th. Pathological anatomy shows that each part of the encephalon may be attended with suppuration, or be destroyed, without any chronic injury of the understanding.

8th. From these data it may be concluded, that there are some forms of insanity, the immediate cause of which defies all our means of investigation; that insanity depends on an unknown modification of the brain; that its origin is not always in the brain, but in those centres of sensibility placed in different parts of the body, as disorders of the circulation do not always depend on lesions of the heart, but on those of any other portion of the circulating system.

4. *Prognosis of Insanity.*—Imbecility and idiocy are never cured. Monomania and lypemania are curable when they are recent, the result of accident, and do not depend on any organic lesion. Mania is cured more frequently than monomania and lypemania. Acute dementia is sometimes cured, chronic, very rarely, senile, never. Hereditary insanity is curable, but relapses are more to be feared than when it is the result of accident. Chronic insanity is with difficulty cured, especially after the second year, and the difficulty becomes the greater, the longer the predisposing causes have been in operation before the attack took place. Of however long standing insanity may be, there is hope of recovery so long as any appreciable derangements of the nutritive functions remain. The speedy action of moral causes in producing the disease is a favourable circumstance, but if it has been slow, recovery is difficult. Insanity arising from excessive study is scarcely curable, especially if there have been deviations from a proper regimen, and when it originates in or is fostered by religious

notions or pride, it is rarely cured. It is also very difficult to cure when aggravated by hallucinations. Those forms of insanity in which the patient has a proper notion of his state, present many difficulties if a recovery do not speedily take place. When assimilation, sleep, and appetite, are natural, and the patient becomes fat without any diminution of the delirium, there is little hope. When the sensibility of the patient is so far weakened that he can gaze on the sun, has lost the sense of smell and taste, and is insensible to the inclemency of the weather, he is incurable. Insanity is incurable when it is the result of scurvy or epilepsy, and a complication with these diseases or with paralysis leads inevitably to death.

5. *Treatment of Insanity.*—The indications in the treatment of the insane are to put an end to their physical disorders, the aberration of their understanding, and the turbulence of their passions. A combination, therefore, of physical and moral means must be had recourse to, and we must never lose sight of the causes which predisposed to and produced the disease; the habits of the patient; his previous disorders, and those which occurred immediately before his attack, or ceased on its appearance. In enumerating the symptoms, it has been stated, that a morbid state of sensation, of association and will, produced by deficient attention, as well as disorder of the passions, induce and keep up delirium. Everything which may act on the brain, directly or indirectly, and influence the mind, everything which may overrule and direct the passions, will be the object of moral treatment.

a. *Isolation.*—The first question which presents itself respects isolation. Ought the patient to be withdrawn from his habits, mode of life, and those with whom he lived habitually, to be placed in a place unknown to him, and confided to the care of strangers? The English, French, and German physicians answer in the affirmative, and undoubtedly experience shows their opinion to be correct.

The first effect of isolation is to produce new sensations, and to change and break the chain of ideas from which the patient cannot escape. Thus the moment he is isolated he is surprised, astonished, and disconcerted, and a remission takes place which is valuable to the physician, who may then easily acquire his confidence. Isolation is beneficial, too, in combating disorders of the affections. When the patient is left at home, he is persuaded that all are bent on contradicting, annoying, betraying, and injuring him. Restraint by those whom he has been accustomed to command aggravates the disease. Besides, the cause of the evil may be some domestic circumstances, and the sight of the house and of the persons inhabiting it serves to foster and increase his delirium. It has been frequently observed, that while the greatest aversion is entertained by the patient to his dearest friends, he is agreeable to strangers, and makes every effort to conceal or shake off his delusions. Such are a few of the reasons for adopting isolation as a primary part of all plans of treatment.

M. Esquirol recommends the patient to be sent to an asylum, and not to a private house, where the expense is great and the isolation incomplete. Such partial separation is rarely successful. The most specious objection to sending the patient to an asylum, is that founded on the assumption that living with others similarly affected must necessarily increase the disease. M. Esquirol's extensive experience, however, leads him to state that in general no such result occurs, and that, on the contrary, the mixing with other insane patients contributes to the cure, by rousing the attention. The novelty of the scene, and the extravagant conduct of his fellow patients, prevents the insane individual from living as it were within himself, forces him to forget himself and his ideas, and occupy his thoughts with what is passing around him, and thus one step is gained towards recovery. A desire of being liberated, and an anxiety to see his relations and friends, supersede his imaginary desires and wants. Even tedium, in its own way, exerts a favourable influence on the ideas and affections of the insane, and the presence and conduct of their fellow patients serves as a text to the physician who wishes to address their imagination. It cannot be denied, however, that there are cases in which isolation, like all useful things, may be injurious, unless it be modified according to the susceptibility of the patient, the character of his delirium, his passions, habits, and manner of life. No absolute rule of practice can be given. The art lies in distinguishing those indications which ought to modify the principles, whatever force they may derive from experience.

In an asylum the patient is more carefully watched, and with less restraint than in a private house. What can be done with a furious individual in an apartment or in a house, however large it might be? For the sake of his own preservation, it would be necessary to tie him down to his bed, which would increase his delirium and fury, while in an asylum he might indulge in his incoherence with less danger to himself and others. There, too, the management is better understood, the servants are more experienced, and the arrangement of the building allows the patient to be removed from one part to another according to his condition, the efforts which he makes, and his approach to reason. These statements, which are applicable to the rich, are more so to the poor.

The asylum ought to have regulations to which all must submit, which may serve as an answer to all objections, assist in overcoming all opposition, and afford motives to obedience less repugnant than the will and caprice of a chief. In such an establishment there is a sort of movement and activity into which each by degrees enters. The most obstinate or the most diffident lypemaniac is hurried away by the general movement and by the impressions, frequently absurd, made on his senses. Even the maniac, restrained by the harmony, rule, and order of the establishment is more on his guard against his impulses, and indulges less in acts of eccentricity.

In an asylum there ought to be a chief from whom everything should proceed. This is contrary to the opinion of Riel and others, who think that an asylum ought to be governed by a physician, a metaphysician, and a moralist. Such writers can have had no practical information, or they would have formed a better estimate of the inconveniences and evils attending a division of powers in the management of such institutions. We shall make some farther remarks on this subject in our subsequent paper on the statistics and management of the insane. In the meantime, we may observe, that we perfectly coincide with M. Esquirol in thinking that there ought to be only one individual invested with full controlling powers in an asylum, and that that individual should be a medical man. Whether he be responsible to a board of physicians, to a body of directors, or to the home government, for the exercise of his powers and the performance of his duties, his authority within the house ought to be absolute both over the patients and the attendants. The servants ought to set an example of obedience to his orders, which will have a most important influence on the insane. Their number exhibits an amount of power which overcomes the most furious patient, and very frequently renders the employment of force unnecessary. A patient might be tempted to resist one or two; but when he sees a body of four or five calm, powerful, and determined individuals, ready on the instant to execute the orders of the superintendent, the folly and fruitlessness of any resistance, becomes too apparent to allow him to make any effort.

Example, which has such power over the determinations of men in general, exerts a great influence over the insane. We must bear in mind that the insane frequently exhibit great sagacity in judging of what passes around them. The cure and departure of one patient inspires others with confidence, the hope of recovery, and an assurance that they too will be liberated when cured. The convalescent patients soothe and console the others, and, by their conversation and advice, are of great service. Thus the inhabitants of an asylum react with advantage on each other, and everything contributes to their cure, care being taken that they can injure neither themselves, their fellow patients, nor their attendants. Let not those who arrogate to themselves the title of practical men sneer at or disbelieve these statements. The plan has been put to the test of experiment by men who are practical in the true sense of the word, and has been perfectly successful. What has been once done may be done again; and we see no reason why that which has been proved by the test of experience on a large scale, and for a number of years, and found to be useful, should be rejected in consequence of the hypothetical objections suggested by the obstinacy and ignorance of a few self-styled practical men, who have, in the course of their lives, probably seen fewer patients than M. Esquirol and others in a single year.

The calm which patients enjoy when removed far from all tumult and noise, the moral repose which their withdrawal from business habits and domestic affairs procures them, the necessity of restraining themselves, the sight of companions in misfortune, are favourable to recovery. The care bestowed on them in the bosom of their own families makes no good impression on them, while the attention which they receive from strangers is appreciated from its being new, and their having no right to expect it. Soothing conduct and kindness have a powerful effect, and if an experienced and skilful attendant profits by this, and commands confidence and esteem, the patient will soon find that he must conform himself to his wishes, and secure his good-will. The necessity of thus submitting himself to another, hope, fear, even the tedium which he feels, force him by degrees to suspect that he is ill, and when he is fully convinced of this, recovery is not far distant.

Some insane persons when transported to a new situation believe themselves abandoned by their relations and friends. If, in these circumstances, they be consoled and kindly treated, and a promise be held out of promoting a return of that affection which they consider lost, they pass from a state of despair to one of hope. The presumed neglect of friends, and the kind and benevolent care of strangers, excite in their minds a struggle in which reason is sometimes victorious.

Others imagine that they have been brought to their new habitation to be given up to their enemies, or to undergo punishment. If their fears be removed by obliging and affable conduct on the part of those by whom they are surrounded, speedy recovery may be predicted.

Thus both reason and experience show the necessity of isolation as a preliminary and auxiliary means of cure.

But it has been said that, by living constantly with madmen, the most rational men may become insane; that patients may injure each other; that it is impossible to conceal from the convalescent the state in which he has been; and that no good can result from separating from the objects of his affections, an unfortunate being overwhelmed with sorrow, or from shutting up a man who is in fear of being imprisoned, &c. Such objections are fully answered by what has been said above. Recovery undoubtedly takes place when the patient is treated at home. But the cases, however, are rare. It may also be admitted that isolation is in some cases most prejudicial. But this is no objection to the plan of treatment. It only shows the necessity of not acting empirically in insanity, any more than in other diseases. All plans of treating disease require to be modified in particular circumstances, and it is the business of the physician to judge of this.

M. Esquirol finds it impossible to state the length of time during which isolation should be continued. As a general rule it may be laid down, that when after a reasonable time it has produced no amendment, the visits of relations and friends may be encouraged, great prudence being used in the choice of those who are admitted. The visits should be sudden, for the purpose of rousing, if possible, the patient. Great caution, however, is necessary in permitting convalescents to be seen by their friends, for prolonged isolation is less dangerous than premature visits.

It must be borne in mind that, at the commencement, the delirium of insanity has a great resemblance to that of fever; that it is not easy to distinguish them; and that isolation in the latter case would endanger the safety of the patient, and the reputation of the physician. In all doubtful cases, therefore, some days ought to be allowed to elapse, that the characteristic symptoms of insanity may have time to manifest themselves.

In dementia and idiocy, isolation is useful only in preventing accidents, giving the patients habits of order, and insuring security. Lypemaniacs sometimes suffer from isolation, in consequence of their excessive susceptibility. But monomaniacs and maniacs must necessarily be confined. Isolation is also necessary in the case of the insane poor, who cannot receive the proper attention at home, and may injure both themselves and others.

Isolation may vary in degree. The patient may travel, and thus be so far removed from objects which excite his disease. He may be separated at home from his friends and relations, he may be transferred to a strange place, and be surrounded by strangers, or removed to an asylum. The first is useful only in very slight cases, and at the commencement, or when the patient has recovered, and it is necessary to introduce him gradually into society. In most cases, with very few exceptions, the last is the best.

b. *Moral Treatment.*—Isolation is in almost all cases a necessary preliminary and accompaniment of moral treatment. It acts directly on the brain, withdraws the patient from irritating impressions, represses excessive vivacity and impressibility, and moderates the excitement of the intellect and the passions. By reducing the sensations of the maniac to the smallest possible number, his attention is fixed on sudden and frequently repeated impressions, and by rousing the monomaniac and lypemaniac from their fixed ideas, and forcing them to attend to objects foreign to their thoughts and their supposed distresses, and by exciting the enfeebled attention of those affected with dementia, a step is gained towards recovery. Such effects, however, can be obtained only by unexpected events, by lively, animated, and brief conversations, and not by long trains of argument, which are never useful. The language of truth, sincerity, reason, and benevolence must always be employed, but no one who knows any thing of the treatment of insanity will employ syllogisms or long trains of reasoning. "I understand you perfectly," said a young lypemaniac to Esquirol, "I comprehend your reasoning. If I were convinced by it, I should be cured. I know all that," exclaimed another. "I wish to do as you desire, but give me the force, the power of which I am destitute, and you shall have me cured." This is the time for employing the disturbing plan *(methode perturbatrice,)* for breaking one spasm by another, and producing strong moral impressions, which dissipate the clouds with which the intellect is overcast, tear asunder the veil interposed between the patient and the external world, break the morbid chain of ideas, put an end to morbid associations, destroy their desperate intensity, and break the spell which binds the active powers of the insane. This object is attained by rousing the attention of patients sometimes to new objects, at other times by surrounding them with phenomena which astonish, sometimes by placing them in a state of contradiction to themselves, and occasionally by entering into their ideas and caressing and flattering them. By entering into their feelings, their confidence is gained, which is a sure pledge of their approaching recovery. Some patients require their character to be entirely subdued, their passions to be bridled and their pride humbled, while others must be excited and encouraged. The unruly transports of the insane must be repressed, and the depressed spirit of the lypemaniac sustained. One passion may be opposed to another, and reason will sometimes come forth victorious from the struggle. The patient is sometimes harassed with fear, and some do not sleep from being roused by panic terror. Encourage them by permitting some one to sleep in the same room, and leave them a burning candle during the night. It is of great consequence to substitute a real for an imaginary evil. A monomaniac tires of everything, though he is in profusion. Separate him from his habits, impose on him real privations, and his dissatisfaction, which is then reasonable, will be a powerful means of cure. A lypemaniac believes himself abandoned by his friends; separate him from them, deprive him of the testimony of their affections which he overlooks, he will then regret them, be anxious for them, and the consequent distress which is well founded, will lead to recovery. Many other illustrations might be given; but these are sufficient to establish the principle.

Though M. Esquirol considers that the good effects of music were exaggerated by the ancients, and that experience has not shown all the cases in which it may be useful, he is of opinion that it is a valuable means, especially during convalescence, and ought not to be neglected, though the principles which should regulate its use and its efficacy be somewhat uncertain.

The distracting of the attention, next to employment, is the most efficacious means of curing the insane. But those circumstances which excite the imagination and the passions are to be avoided. The lypemaniac, always jealous, converts whatever strikes his senses into nourishment for his disease; the maniac is excited by a display of the passions, by the vivacity of conversation, and by the excitement of theatrical representations, which indeed to every class of the insane are detrimental. These, therefore, M. Esquirol discards, and considers to be unsuitable even to convalescents.

Travelling is another moral means of treatment. M. Esquirol has constantly observed that the insane are soothed by a long journey, especially when they have visited distant countries, the appearances of which made an impression on their imagination, and when they have experienced the difficulties, trials, annoyances, and fatigue incident to travellers. Travelling acts beneficially also in a physical point of view, by improving digestion, promoting sleep, and increasing the appetite and the secretions. It is, as we have already observed, particularly useful to convalescent patients, especially those who dread entering again into society, as it affords them subjects of conversation with their relations and friends.

c. *Physical Treatment.*—The general principles on which the physical treatment of the insane should be conducted are not so easily laid down. If the primary cause of the disease could always be ascertained there would be less difficulty. Acting on the brain and nervous system will not always be successful; and attention must be paid to all the organs and functions of the system. The physical means are either hygienic or pharmaceutical.

The constitution of the insane is speedily weakened. They contract affections of the skin, swellings of the lymphatic glands and scurvy,—circumstances which prove the necessity of being careful in choosing a proper situation, and adopting a proper plan of construction, for asylums. The site on which the building stands ought to be exposed to the south-west in France, to the east in hot climates, and to the south in northern countries. The soil ought to be dry and well aired. The apartments ought to be dry, warm, and well ventilated, because, as has already been observed, the insane are susceptible of all physical influences. The clothing, especially of lypemaniacs, ought to be warm; flannel should be worn next the skin, and dry friction had recourse to. The bed should consist of a woollen mattress; one composed of hair, a bolster and a pillow of the same material, and the coverings should be light, and the head usually bare.

The food must vary according to the nature and period of the disease. At the commencement spare diet is necessary, afterwards the quantity and quality may be modified. The dishes ought always to be simple, and prepared without spices. During convalescence a more substantial allowance may be made; and in some rare cases it may be more abundant. The food for the day ought not to be served out all at once, because the patients are apt to devour it the moment it is received, and, being tormented by hunger and thirst during the rest of the day, become more furious or more sad, and are persuaded that their wants are not attended to, and that the attendants wish to starve them. Most maniacs and monomaniacs are harassed by incessant thirst. This ought to be satisfied several times daily. Nothing of an exciting nature, in the way of meat or drink, ought to be allowed at any period of the disease, except in some rare cases of dementia, monomania, and convalescence.

The secretions and excretions are to be promoted by all possible means, and the state of the bowels must be carefully observed, as constipation, which is a frequent symptom, increases the delirium. Exercise of various kinds ought to form a part of the hygienic treatment. Cultivating the ground may in a certain class of patients be advantageously employed instead of other exercise. The celebrity which one of our farmers obtained for curing the insane was owing to this. Bourgoin, in his travels in Spain, remarked that the insane patients in the hospital of Saragossa, who were rich, were never cured, because they could not be forced to work, while those who were poor recovered. Pinel was of opinion that an asylum should always have a farm for the patients to be employed on. Esquirol has found the cultivation of a garden succeed in some cases. At Salpetriere, the best effects are produced by the labour in which the insane female patients engage. They are assembled in a large room, where they sew or knit. Some of them are employed in the service of the house, and others in the garden. Walking, music, reading, parties, &c. form a very disadvantageous substitute for exercise. In rich patients, both male and female, there is a habit of idleness which counterbalances all the advantages which they otherwise possess.

In laying down the principles of the therapeutical treatment of insanity, it is necessary to know all the general and special causes of the disease, to be able to distinguish by certain signs, the origin of all the disorders incident to it; to determine whether the body is acting on the mind or the mind on the body; to ascertain those forms of the disease which recover spontaneously, and those which are cured by moral means, by medicine, or by a combination of both. Physicians who consider all forms of insanity to be the same affection meet with difficulties and obstacles in their attempts to cure it. Influenced by the hypothesis which they may have formed, some see nothing but inflammation, and carry bleeding to an unwarrantable extent; others believe the function of the liver to be disordered, and are prodigal of emetics and drastic purgatives; while others, directing their attention only to the nervous system, exhibit antispasmodics to excess. Such practitioners take no general views of the disease, and forget that it is only by an attentive consideration of all the circumstances connected with it that any rational and successful plan of treatment can be devised and executed.

The physician when called to an insane patient ought first to ascertain the previous history of the patient, the predisposing and existing causes, and what indications it is necessary for him to fulfil. The cause of the disease indicates the plan of treatment to be followed; for instance, if the catamenia be suppressed, the discharge must be re-established, if any cutaneous affection has been repelled, it must be re-excited, &c. If there be great excitement and plethora, it will be necessary to bleed the patient, and keep him long in warm emollient baths. Cooling and laxative drinks must be exhibited, and abstinence or a spare diet enjoined. In some cases rubefacients will be necessary. If the affection be thus treated as an acute disease, at the end of eight, fifteen, twenty-one, or thirty days, a remission, and sometimes a very marked intermission, will be found to take place, and then, by a combination of moral and physical means suitable to the character of the delirium, the material, hygienic or pathological causes, which have predisposed the patient to the disease, must be combated.

When these general predispositions, and the effects of particular causes, have been surmounted, it may become necessary, if the disease be not cured, to have recourse to an empirical or experimental plan, and so far the various means found useful by experience may be varied incessantly. These means will be discussed when we examine the treatment applicable to each form of insanity. In the meantime, we give M. Esquirol's views regarding those remedies which have been regarded as all-powerful in insanity.

a. *Water.*—Water has been used in every way, and of every degree of temperature at all bearable. Baths at the temperature of 20 or 25 C. = 60° or 77° F. continued for several hours at a time are most useful to nervous and very irritable patients; and when there is a great determination of blood to the head, cold applications may be employed at the same time. Young, strong, and robust patients, as well as those tormented with heat, are benefited by the cold bath, the cold acting by subtracting in some measure the excess of heat, and as a tonic to the skin. Hot baths have been recommended by some, and are perhaps too much neglected. Immersion and affusion are useful in weak subjects, especially those who have been debilitated by masturbation, or by long continued grief. M. Esquirol justly reprobates the bath of surprise, which consists in suddenly plunging the patient into a reservoir of water. The surprise and fear produced have been supposed to be useful. It is, however, a very hazardous proceeding, and has proved fatal. M. Esquirol declares that he would as soon think of throwing a patient from the third story of a house, because there are instances of madmen being cured by a fall on the head.

The *douche* acts both by the cold and the percussion which it produces. It excites a sympathetic action in the epigastric region, produces cardialgia, and a desire to vomit, and patients become pale and sometimes yellow after having been subjected to it. It acts morally as a means of repression. One *douche* is frequently sufficient to calm the fury, break the dangerous resolutions, and insure the obedience of the patient. Some who are young, active, and strong, are anxious for the *douche*, on account of the agreeable feeling of coolness which they experience in the head after its use, and which is often very useful, especially where there is cephalalgia. It should never be administered after a meal, the *primæ viæ* should be unloaded before it is prescribed, and it should not be continued for more than a few minutes. Its administration should never be intrusted to servants, for it may be abused by them, and its abuse is not void of danger.

Ice applied for a length of time to the head relieves cephalalgia and calms fury, bleedings, general baths, and the douche, especially when there are symptoms of cerebral congestion. The efficacy of this application is increased by placing the feet of the patient in hot water, or surrounding them with irritating cataplasms.

Warm *pediluvia*, with the addition of muriate of soda, ammonia, or mustard, by acting as revellents, are often beneficial. Care must be taken that they are not too hot, otherwise the pain which they produce may react prejudicially on the brain. Cramps are relieved by placing the legs for a length of time in some emollient decoction gently heated.

Sprinkling or squirting water in the face is useful in cases of stupor. Enemata of water, either plain or combined with purgatives, anodynes, or antispasmodics, according to the indications, has been prescribed. The *douche* directed into the rectum has also been employed as a means of unloading the large intestines, overcoming spasms of the canal, sometimes of giving tone to it, or acting as a derivative.

Avenbrugger and Hufeland gave draughts of cold water in the doses of a tumbler every hour; and Leroi of Antwerp has published a paper on its advantages as a prophylactic against suicide. Many facts seem to justify the practice. The most interesting is that of Theden, a distinguished Prussian surgeon, who, having been hypochondriac in his youth, fell into a state of melancholy, with a propensity to suicide. The copious use of cold water restored him to health. He drank twenty-four and thirty pounds daily. Theden, at the age of eighty years, published the fact, that he continued from habit to drink several pounds of cold water daily. Two cases are mentioned by Hufeland in corroboration.

M. Esquirol has offered no explanation of the manner in which cold water acts in such cases. We would hazard the conjecture, however, that its action was fourfold; 1*st*, It would act locally on the stomach, and indirectly through the medium of the nerves on the brain as a sedative; 2*d*, It would dilute the acrid saburræ which, in many instances, irritate the stomach; 3*d*, It would promote, by the subsequent distension of the vessels which its absorption would produce, the various secretions and excretions; and, 4*th*, It would act beneficially by preventing the patient from indulging in food to any extent, and thus forcing on him unwittingly a degree of abstinence which is always beneficial in hypochondriacal cases.

b. *Emetics.*—Evacuations have long been celebrated as the basis of the treatment of insanity, and especially of lypemania. But, far from being useful in all cases, they sometimes aggravate the evil.

Modern writers have recommended emetics, which are very useful in some forms of monomania, and in some cases of lypemania with stupor. They are beneficial to patients whose sensibility is impaired, and who appear to be in an atonic state, while they are prejudicial in all cases in which erethismus exists. They may be repeated for several days running. They act either by producing sensible evacuations, by exciting perspiration, or by putting an end to spasms of the abdominal viscera by the shock which they cause.

c. *Purgatives.*—The choice of purgatives is a matter of importance. Those which influence the liver act on the rectum or expel worms, will be preferred according to the indication to be fulfilled. The irritation and the suspension of the cutaneous exhalation which they produce, may be prevented or alleviated by alternating them with the hot bath. Many insane patients believe their health to be good, and refuse all medicine. Their repugnance may be overcome by giving them unawares something which will irritate the stomach and intestines, and produce pain or evacuations. The patients, then alarmed about their health, become docile.

Hellebore, gamboge, bryony, aloes, calomel, and, above all, tartar-emetic and purgative mineral waters, are the most valuable medicines of this class. M. Chrestien, a celebrated practitioner of Montpellier, recommends colocynth rubbed on the abdomen, as a sure purgative and effectual means of curing insanity. M. Esquirol tried it in some twenty cases, and did not succeed in curing the patients, or even in procuring any evacuation, except in two cases after parturition.

d. *Blood-letting.*—When the circulation of the blood was first demonstrated, the cause and cure of all diseases were supposed to be discovered, and blood flowed in torrents. That of the insane was the less spared, from its being believed, that, by bleeding them to syncope, they were cured. The error was not perceived, because those whom they injured by bleeding, falling into a state of the most profound dementia, passed for being cured. This treatment was extended to all forms of insanity, on the principle that the blood was too abundant, or too hot, and therefore ought to be evacuated or cooled. Thus in the French hospitals, where some little attention was paid to insane patients in spring and autumn, they once or twice lost blood, they were bathed in cold water, or thrown, tied hands and feet, into a river or cistern. If any recovered after this blind treatment, it was looked upon as a miracle, as well it might. The prejudice in favour of bleeding was such, that it is not a great many years since M. Esquirol heard of pregnant women, requiring to be taken to Salpetriere, being bled by way of precaution, before going to an establishment where bleeding was proscribed. As an instance of the excess to which it was carried, he mentions the case of one patient who was bled thirteen times in forty-eight hours! Pinel opposed himself vigorously to this truly insane mode of treating the insane, and has given some striking cases which should be remembered by practitioners; Esquirol adds, that he has several times seen insanity aggravated by an abundant flow of the catamenia, hemorrhage, and even after one, two, or three bleedings. He has seen sadness soon after bleeding passing into mania and fury, and mania into dementia. Not that he proscribes bleeding in all cases. He considers it indispensable in plethoric subjects, when there is great congestion in the head, and when habitual hemorrhages or bloody evacuations have been suppressed. At the commencement of insanity, if there be plethora, suppression of any hemorrhage, or a determination of blood to the head, the patient may be bled one, two, or three times; leeches may be applied to the region of the jugulars or temples, and cupping over the back of the neck, and lower part of the cranium may be employed. At a later period, local bleeding may be had recourse to, as a revellent, or as a succedaneum for suppressed evacuations.

e. *Tonics and Antispasmodics.*—Tonics and antispasmodics are useful in many cases. Camphor, mezereon, quinia, and antimony are often employed in large doses as specifics, but improperly. They are useful in certain cases, and have a wonderful effect when judiciously prescribed. Indiscriminately used, they are hurtful and dangerous.

f. *Narcotics.*—In consequence of some insane patients passing nights, weeks, and even months with little or no sleep, recourse has been had to narcotics. These medicines, however, are productive of more evil than good, especially when there is plethora or determination to the head. Valsalva and Morgagni long ago objected to opiates, and daily experience proves the accuracy of their opinion. Proper regimen, labour, and exercise are the true means of promoting sleep. The hot or the cold bath is also useful, and is attended with no danger.

g. *Counter-irritation.*—Setons, moxa, the actual cautery, dry cupping, friction with irritating and mercurial preparations, are excellent means of exciting the sensibility of the skin, which is often deadened, of procuring revulsion and general reaction, besides being good substitutes for suppressed cutaneous affections, &c. It has been proposed to apply a saturated solution of tartar-emetic, or epispastic plasters over the whole scalp. This is often prejudicial, from its irritating the patients, and persuading them that they are tortured or punished. Such active measures are in general applicable only to cases of monomania or dementia; and M. Esquirol is of opinion, that their success in other cases is very rare, and the circumstances indicating their employment very difficult to determine.

M. Esquirol has sometimes been successful in treating mania complicated with fury, by applying the actual cautery to the nape of the neck. But no advantage was ever gained by him when the patient exhibited any symptoms of palsy. A seton in the same situation he has more frequently found useful in those cases of dementia which are apt to be confounded with idiocy, but only when they were uncomplicated with palsy.

h. *Electricity and Animal Magnetism.*—Gmelin and Perfect boast of having cured insanity by electricity. But the trials made of it by M. Esquirol during the summers of 1823 and 1824, which were conducted on a large scale, seemed to be successful only in one instance. This was the case of a stout young girl, who became maniacal in consequence of the catamenia being suppressed by a fright. She had been insane for a month, and was subjected to electricity for fifteen days. At the usual period the discharge appeared, and recovery followed. It appears to us that no conclusion can be drawn from this case as to the effects of electricity, because in all probability the patient would have recovered on the reappearance of the catamenia, whatever might have been the means adopted to effect this. In 1812, M. Esquirol tried animal magnetism along with Professor Aldini. In two cases the catamenia were restored, but the patients remained insane. In 1813 and 1816, also, he made some experiments along with M. Faria, on eleven women who were maniacal or monomaniacal. Only one, who was remarkably hysterical, was affected by it, but the character of her delirium remained unchanged, and not the least effect was produced on the other ten. No effect attended any of his trials with other magnetists; and he asserts that the facts in favour of animal magnetism reported in France have been neither accurately observed nor faithfully related.

i. ***Rotatory Machine.***—In the first edition of this part of M. Esquirol's work, which appeared in the *Dictionnaire des Sciences Medicales*, he stated that this machine, when used prudently, might be useful in some cases in which the patients exhibited gastric symptoms, and refused all kinds of medicine. He does not appear to have employed it in his own practice, and in a note in the volume before us, he informs us that it is everywhere abandoned. From this we infer, that he now withdraws the very guarded sanction which he formerly gave it.

From a careful consideration of all that has been written both in favour of and against the employment of this machine, as well as for several physiological reasons, which will easily suggest themselves to our readers, we have no hesitation in condemning it as useless in all cases, and dangerous in many. We rejoice, therefore, to find that its employment has been abandoned.

M. Esquirol concludes this part of his work by noticing those means which ought to be adopted to prevent the appearance of insanity in those predisposed to it. These are general or special, and have already been indicated in the preceding remarks.

Marriage between individuals, the offspring of insane patients, must be avoided.

The education of man commences in the cradle. No stories should be told to children which are calculated to shock their understandings, or terrify their imaginations. In cultivating the infant intellect, the affections ought not to be neglected, and it ought never for a moment to be forgotten that education consists less in what is learned, than in acquiring proper habits of thought and action. If education be neither religious nor moral, if the child encounter no obstacle in the attempt to gratify his wishes and caprices, if everything which he desires be granted, how shall he be able to encounter the obstacles and misfortunes with which the path of life is beset. Sensibility and intelligence are not to be developed by acting too early on the brain, or fatiguing it with long and abstruse action. These too often predispose to or induce insanity, and ought to be avoided. Let the intellect be suitably cultivated, the passions repressed, and directed to proper objects.

The education of those born of parents affected with, or predisposed to, insanity ought to be less intellectual than physical and gymnastic. The teacher, forewarned of the state of the intellect of the parents, ought to treat the pupil accordingly, moderating his vicious or excited disposition, and fortifying him against the assaults of his passions; while the physician, made aware of the physical causes which have excited the disease in his ancestors, should endeavour to prevent their developement and action, and modify their effects as far as possible by regimen and proper medical treatment.

The convalescence of a patient must be secured, and relapses guarded against by confining him to a style of living suitable to his constitution, and the causes and characters of the form of disease from which he has been relieved. He must be guarded against the influence of the predisposing, moral and physical causes of his malady, and prevented from exposing himself to any errors of diet, excessive study, or indulgence in any passion. Experience shows that relapses frequently take place from the simultaneous developement of moral and physical causes, and that those causes ought to be combated without waiting for an attack of insanity. An emetic, or a purgative, may prevent it. Leeching on the slightest disturbance of the menstrual affection may save the patient from an attack. If the suppression of a cutaneous affection of gout, a habitual discharge, or rheumatism, has preceded the first attack, such suppressions or metastases must be guarded against. The same principles apply to the moral treatment.

If this prophylactic treatment were vigorously adopted, the predisposition to insanity would not be so hereditary as it is.

We have now in a general manner, and with little or no comment, laid before our readers M. Esquirol's views of insanity. Considering them generally we give them our hearty concurrence. The remainder of the work, containing the developement and application of his principles, we reserve for further consideration, when we shall have an opportunity of discussing, one by one, the various subjects, of pointing out where he has fallen behind the age which is so much indebted to him and his master, Pinel, for its advancement in this department of medical science, and of showing in what particular cases important truths developed by him have been neglected or misunderstood. In the meantime, we recommend all who are interested in this department of medicine to study carefully and leisurely the volumes before us.

ART. II.—*A Practical Treatise on the Management and Diseases of Children.* By RICHARD T. EVANSON, M. D. Professor of Medicine in the Royal College of Surgeons, Ireland; and HENRY MAUNSELL, M. D. Professor of Midwifery in the Royal College of Surgeons, Ireland. 2d Edition, enlarged. Dublin, London, and Edinburgh, 1838. 8vo. Pp. 483.

THE diseases incident to the period of infancy and childhood are peculiar in this respect, that in their origin they are often obscure and insidious, in their course difficult to be detected, and in many circumstances so complicated, that they not unfrequently require a very practised eye to discover their existence and conduct their treatment. To whatever extent, therefore, they may have been admitted into the usual systematic works on the Practice of Physic, and however true be the axiom, that disease in the child and in the adult is always deviation from the state of health, and that the treatment of diseases at all ages must be conducted on the same general principles, the most confident advocates for the unity of medical science and art must admit the necessity and utility of bestowing special consideration on the disorders most common to the infantile period of life.

It was early, therefore, recognized, that the best methods of preserving the health in children required particular consideration; and the treatise περι φυσεος παιδιου, ascribed to Hippocrates, and composed by one of the disciples of his school, may be adduced as a proof of the conviction of the necessity of devoting particular attention to the numerous diseases incident to this early age.

In modern times, we find that the management of infants, and the cure of their diseases, have attracted not less attention; and this has progressively increased with the improvement and advancement of the healing art. James Rueff, an eminent physician and lithotomist of Zurich, was among the earliest writers who in modern times attempted to instruct the profession in the cares requisite to the proper management of children;* and he was followed in the ensuing century (1644,) by a French midwife, Louise Bourgeois or Boursier,† who acted in that capacity to the Queen, and had been instructed by Ambrose Paré. In general, indeed, obstetrical writers introduced into their works directions, more or less lengthened and detailed, on preserving the health and curing the ailments of infants.

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* Hebammenbuch daraus man alle Heimlicheit dess weiblichen Geschlechts erlehrnen könne sampt Anhang von Cur und Pflegung der Neugebornen kindtlein· Frankfurt, 1588. 4to.

† Observations, &c.—et Maladies des Femmes et Enfans nouveaux nés. Paris, 1648.

Zuinger, in 1722, and Andry, in 1741, published express treatises; and Walter Harris of London published in 1736 a good treatise on the acute disorders of infants. Besides these appeared the comprehensive work of Rosenstein, the essay of Frank, the judicious observations of Cadogan, and the useful compendium of George Armstrong; and at a later period the work of Petit, the practical observations of Underwood and Gustavus Hume, the useful epitome of Heberden, the original and instructive writings of Cheyne, and Dr John Clark, and the elaborate treatise of Capuron, were added to the list. In Germany a great number of essays and tracts on the peculiar diseases of children have been published; and recently these have been collected and republished in one continuous work.

The multiplicity of works now accumulated on this subject is attended with great and increasing inconvenience to the student and practitioner, who become bewildered amidst the number and variety of treatises, and often are perplexed, which to chuse as a useful guide. The most comprehensive of the whole of the works published in England, namely, the interesting essays of Underwood, has very generally been resorted to, and recommended as a useful practical compendium; and in order to adapt it to the advancing progress of knowledge, this work was republished in 1827, by Dr Merriman, with various additions and improvements; and it must be allowed to be both a respectable and useful treatise.

The advanced state of knowledge, nevertheless, required a work not less comprehensive and more condensed; with all unnecessary and luxuriant incumbrances removed, and with all needful information supplied. In this respect, while it was requisite to make it a work chiefly for the English reader, it was proper to incorporate whatever useful information had been furnished by foreign authors. The present work, of which the first edition appeared in 1835, comes very conveniently to supply the wants of the English reader in this respect; and we propose to say a few words on the points in which it appears to us most to deserve attention.

This work is the result of the conjoined labours of two gentlemen who have been connected with a large institution for the treatment of the diseases of children in Dublin, ever since the period of its establishment, first in the capacity of pupils, and secondly, in that of medical attendants. It is manifest that such opportunities could not fail to afford ample means of observation and experience; and, with ordinary attention on the part of those who enjoyed them, were in all respects the most favourable circumstances; which could qualify the authors for undertaking the useful task of instructing their coeval and junior brethren in a branch of medical practice of the highest importance, and in many respects, very imperfectly understood.

All impartial and enlightened judges, who know what ought to be the qualifications of a work intended to instruct the student and to guide the practitioner, will admit that the principle observed in the composition of the present treatise is the one most likely to insure the production of a useful and convenient book. Taking, as the general basis of their performance, the practical knowledge acquired, during a course of several years of assiduous observation, by the authors themselves, and adopting whatever the experience of contemporaries, at home or abroad, had added to the general stock,—they naturally believed that they might thus produce a work which, within moderate compass, should present a reasonable degree of information on all the diseases of the infantile period most usually requiring the attention of the practitioner, and furnish, at the same time, rules for the treatment of those which admitted treatment, according to the most improved methods.

The work in its present form consists of thirteen chapters, six of which are composed by Dr Evanson, and seven by Dr Maunsell. The method of arrangement, and the general plan of the work, may be understood from the following general sketch.

The first chapter by Dr Evanson is devoted to the consideration of the Peculiarities of the Infant Structure and Constitution. In the second, Dr Maunsell treats of the Management and Physical Education of Children; and in the third, of their Mental and Moral Education. In the fourth chapter, under the title of Peculiarities of Disease, Dr Evanson considers the Etiology, Diagnosis, and Prognosis of Infantile Diseases in general; and in the fifth, on Infantile Therapeutics, he takes a view of the various remedial agents employed in their treatment. In chapter sixth Dr Maunsell gives an account of the accidents and diseases occurring at birth or shortly afterwards. In the seventh chapter Dr Evanson gives a history of the process of dentition; and in the eighth a view of the diseases incident to the Digestive Organs. The ninth chapter, devoted to the Diseases of the Respiratory Organs, is by Dr Maunsell; and the tenth, on the Disorders of the Brain and Nervous System, is by Dr Evanson. Lastly, the eleventh chapter, on Eruptive Fevers, including Measles, Scarlet Fever, Small-Pox and Chicken-Pox; the twelfth on Vaccination; and the thirteenth on Constitutional Disorders, embracing Scrofula, Rickets, Syphilis, Purpura, and Pemphigus, are all by Dr Maunsell.

The nature of this work prevents us from entering into a minute or lengthened analysis of the chapters now enumerated, or an examination of the mode, in which the subjects discussed in each, are considered. We shall merely select a few topics for consideration; and in this selection we shall study to be guided by the importance of the subjects, their novelty, or the originality and value of the information communicated.

Of the first chapter by Dr Evanson, we have little to say, except that it gives a short and clear account of the principal peculiarities on the Structure of the Organs of the Infant.

The second chapter, on the Management and Physical Education of Children, which is more practical in its nature, deserves greater attention. Dr Maunsell recommends, after seeing that the mouth, nostrils, and throat, are cleared from all mucus and foreign bodies, that the infant be placed in a suitable temperature, and sufficiently protected against atmospheric cold. The *vernix caseosa*, also, or peculiar white unctuous viscid matter with which the surface of the infant's skin is coated, he also recommends to be completely removed by careful washing with tepid water. The reason is this, that it is not only a piece of cleanliness, but because the presence of this varnish, however necessary while the fœtus was floating in the *liquor amnii*, becomes unnecessary when it is placed in the atmosphere, and injurious by intercepting the animating influence of the air. After seeing that the cut end of the chord does not bleed, the end he advises to be drawn through a bit of soft linen, with a hole already cut, and the linen then gently folded round it.

On the subject of food or medicine immediately or soon after birth, he is disposed in ordinary cases to allow the infant to sleep quietly for five or six hours after birth, undisturbed by either, and only to give a little castor oil, viz. about half a drachm, perfectly fresh and free from rancidity. The purgative property ascribed by many authors to the first secreted milk or *colostrum*, he is disposed to doubt entirely: and when purging appears to follow it, he thinks that it may be produced by mere mechanical distension. Does any purgative effect, we ask, follow in cases in which the first secreted milk is not allowed to be drawn by the infant? Is it not equally probable, that any apparent purgative evacuations are the result of the new mode of existence, the new stimuli and impressions applied to the delicate tissues of the infant, and the new direction taken by the circulating current? Has the blood circulated through the lungs, exposed to the air, and then circulated through the whole body, but especially the vascular texture of the alimentary canal, no influence on its secretions and on the peristaltic motion of its muscular coat?

On the subject of food in the first period, though Dr Maunsell gives the preference to the mother always acting as nurse to her infant, he allows that certain circumstances take place, which, either physically or morally, disqualify the parent from performing this duty; and, under such circumstances, he properly recommends that a good healthy nurse, with calm tranquil temper, be speedily procured. The directions for ascertaining the qualifications of the nurse are extremely good.

The subject of artificial feeding next claims attention. He thinks it in vain to attempt to prepare any kind of food resembling human milk; and, consequently, advises, as the nearest substitute, cow's milk diluted with one part of thin barley water, and sweetened by means of loaf-sugar. As the child advances in age, he allows the use of thin panado well prepared.

The process of weaning, and the management of the diet at that period, is of great importance to the welfare of the infant. Regularity in the times of feeding, Dr Maunsell recommends as of primary importance. During the day an interval of about four hours, he thinks quite sufficient, between any two meals, to give the stomach and *duodenum* time to perform their offices in digestion, and allow the former to recover energy adequate to the chymification of another meal. This principle is important to be kept steadily in view. But the length of time will be found to vary in different individuals; some children, even after weaning, craving food at shorter intervals. The author allows, however, that neither on this point, nor on that of the precise kind of food, is it possible or judicious to lay down absolute and general rules; and he cautions against the adoption of preconceived systems or speculative notions, especially in the choice of the kind of food on which children thrive best.

> "It is of great importance," he says, "that no preconceived system or theory, with respect to diet, should be assumed and adhered to with unreflecting obstinacy, as is but too often the case. Thus one class of persons, arguing that the growth of a child requires an active nutrition, adopt the theory that an excess either in the quantity or quality of nutritious matter cannot be committed; and then, by cramming and overloading the stomach, impair the digestive powers, and produce the defect in supply of new material which it is their object to prevent. Others, observing a healthy remnant of potatoe-fed children in an Irish cabin, and hearing that the Hindoos live and thrive upon rice, instantly infer that scanty vegetable food is the true panacea for health and longevity; and accordingly, condemn their offspring or patients to the miseries and perils of an insufficient, non-animal diet. Most unfounded prejudices are also taken up from *ex parte* observations as to the wholesomeness or unwholesomeness of individual articles of food. Fruits, for instance, are under a general interdict from the dietary of children; and the consequence is, that the school-boy, impelled by his instinct on the one hand, and restrained by the *soi-disant* science of his managers on the other, devours, in secret, the unripe ill-cultivated and pernicious trash of the fruit-stall, instead of being permitted a moderate use of the wholesome, cooling, and antiseptic

produce of the garden. Much learned trifling has also escaped from writers with respect to the propriety and necessity of 'living *naturally*, and on such food as is presented to us by nature:' and we are often told, that children should be fed upon bread and milk alone; because the former is prepared by the simplest cookery, and the latter in what they call the laboratory of nature. Dr Fordyce however, has met the question, and settled it at once by declaring that 'man has no natural food.' Let those who may be startled by this proposition, call to their recollection the difference between the milk afforded by a cow kept alive upon the weeds and scanty vegetation of a neglected waste, and one fed upon the riches of a pasture which has experienced the transforming power of cultivation; let them recollect that the wheat, of which their so-called natural diet is composed, is itself the result of a long course of cultivation, and a full employment of all the resources of the complicated art of agriculture. 'The potatoe,' says Dr Paris, among many other examples—'the potatoe, whose introduction has added so many millions to our population, derives its origin from a small and bitter root which grows wild in Chili, and at Monte Video.'"—Pp. 47, 48.

After impressing the importance of cleanliness, by observing periodical ablution of the surface, Dr Maunsell considers the subject of clothing, and makes several observations which every one who wishes well to children would wish to see more generally known. Though the primary purpose of clothing must be admitted, at least in these and similar climates to be, to protect the surface against cold, humidity, and atmospherical vicissitudes, yet this is too often made a secondary consideration both in old and young, while the primary and essential object appears to be the best means of exposing the person and gratifying vanity.

It would indeed be astonishing how general and how deep-rooted are the errors of many on this point, were it not known that it was one of the few absurd fancies of a very able philosopher, viz. Mr Locke, who, with others chiefly of the stoical and ascetic school, believed in the possibility of hardening the human frame, as if it were iron or steel, to advise that children should, from their youth, be accustomed to cold, hardship, and fatigue. His notions, on this point, Locke borrowed from the ancients, without having any means but what is represented to be experience, to determine the actual merits of these notions.

The Scythians, according to Herodotus, the Spartans, according to Plutarch, and the Persians, according to Xenophon, accustomed their infants to very great degrees of cold, with little or no clothing; and the answer of Anacharsis,—so apocryphal that it may be regarded as fabulous,—is often triumphantly quoted, to prove that the whole human body may be inured to the inclemency of a Thracian or Scythian winter without injury or inconvenience.

" Sudavit et alsit, abstinuit vino et venere puer," has often been quoted to the destruction of many, without adverting to the fact, that it contains a mixture of truth and error. Of the excellence of the latter part of the sentiment, both morally and physically, no doubt can be entertained; but the extremes of heat and cold can be borne by few with impunity; and by none in the early period of life.

The influence of Locke and similar authorities has been ably and happily controverted by Goldsmith, who had certainly good grounds for saying, that the great psychologist was but an indifferent physician (Essay vii.); and who showed the fallacy of his conclusions, by directing attention to the fact, that, among the savage nations, to which these reasoners were so fond of appealing, many children died under the severity of the ordeal to which they were subjected. But the inferences, which common sense and observation enabled Goldsmith to deduce, have been fully confirmed by Dr Edwards in the work referred to, and may now be regarded as incontrovertible principles in physiology.

"If the attentions," says this author, "which children require in climates and seasons little favourable to the preservation of their existence, were generally understood and put in practice, it would considerably reduce one of the most painful sources of mortality affecting that age in our climate. It is not confined to children whom the misery of their parents cannot guard from the rigour of the weather, but it operates to a great extent, without being either perceived or suspected, in families enjoying affluence, and in which it is believed that the necessary precautions are taken,—because, cold being relative, it is difficult from our own feelings to judge of its effects on others, and because it does not always manifest itself by determinate and uniform sensations. They do not feel the cold, but they have an uneasiness or an indisposition which arises from it; their constitution becomes deteriorated by passing through the alternations of health and disease, and they sink under the action of an unknown cause. It is the more likely to be unknown, because the injurious effects of cold do not always manifest themselves during or immediately after its application. The changes are at first insensible; they increase by the repetition of the impression, or by its long duration; and the constitution is altered, without the effect being suspected."—Pp. 51, 52.

The whole passage in the work of Edwards deserves particular attention from those admirers of the inuring system, who tell us that warm clothing makes the human body more susceptible of cold.

Of the justice of the following remarks, we presume most of our readers can readily judge from personal observation in all large towns. After the extract from Dr Edwards already given on the influence of cold in impairing health, inducing disease, and causing mortality among children, (Applications, xviii. p. 265, Hodgkin's translation,) Dr Maunsell proceeds in the following manner.

"We wish we could, as a commentary upon the foregoing passages, adequately depict one of those miserable victims of parental vanity, whose appearance in our streets will sometimes, upon a March or November day, strike cold into our hearts. The cap and feathers set upon, not covering, the child's head, and probably of a colour and richness contrasting mournfully with blue ears, sharpened nose, and shrunken cheeks, in which cold has assumed the features of starvation—the short kilt and Highland hose, exposing between them cracked and quivering knees—altogether require for their description more graphic power than we presume to lay claim to. We hope, however, that we have said enough to call attention to the absurdity of the 'hardening' system, as it is called, and to show that a clothing regulated so as to obviate the rigours of our climate is both demanded by our sensations and sanctioned by our knowledge."—Pp. 52.

The practical application by Dr Maunsell contains so many good directions, stated clearly in a short space, that they are subjoined.

"The material of clothing is not, we conceive, of very great consequence. We are rather inclined to discourage the use of flannel next the skin in children; it is liable to injure the tender and irritable surface, and it is more likely than linen to permit the concealed accumulation of dirt. Upon these accounts, and as children need not be exposed to wet, we think flannel next the skin had better be avoided. The feet should be protected in winter by warm stockings, and sufficiently strong shoes; and when the weather is particularly cold, woollen gaiters should be drawn over the legs when the child is about to be exposed to the air. It has latterly been a good deal the fashion to allow children, when a little advanced, to sleep without night-caps; and this, we think, may be permitted without injury, as the non-confinement of the hair certainly promotes its growth, and in this instance we may safely leave nature to herself. In every article of dress the principle should be carefully followed, of placing no constraint upon the motions of any part. For the boy tight-waisted trowsers or braces, and for the girl stays of all kinds, must be forbidden during the whole period of childhood. The injuries that may be committed upon the organs in the chest and abdomen, by the latter article, are well known to be of the most serious nature; the chest may be completely altered in shape by a continued pressure, and the lungs diminished in their capacity, while at the same time the stomach and liver are driven from their natural position, and made to press upon the other organs of the abdomen. Derangements of the functions of respiration, circulation and digestion, follow as natural consequences, and but too often lead to an early grave, or, what is perhaps worse, a life of prolonged delicacy and ill-health. After all these risks, the object held in view in the use of stays is not attainable by their means. By active and continued compression upon the lower part of the chest, its shape may no doubt be altered,—the ribs may be brought together, so as to form a narrower circle, and a shape be produced which however it may be fancied to constitute the ideal of beauty, yet bears no resemblance to the human form, either as it comes from the hands of nature, or exists in the conception of the artist. The only way in which we can assist in the production of a really fine figure is, to remove all restrictions, and secure, as far as possible, so free an action of the muscles as will lead to their perfect developement. By such a course also, we shall best promote the acquisition of a good carriage, which is infinitely more likely to be the result of a perfect balance of the muscles, than of any mechanical support whatsoever. If the principle of avoiding constraint be held in view in the management of a child's feet, much suffering would be spared in after life. Corns and tumours upon the toes are almost always the results of tight or ill-fitting shoes; and therefore it is highly desirable, that we should have these articles so constructed as to protect the foot equally, and not injure it by any undue pressure upon particular parts."—Pp. 52–54.

The twelfth section of this chapter, on the employment of medicine in the infantile period, deserves attentive perusal and serious consideration. But the matter most to be deplored is, that the excellent observations contained in it, may probably never come before the eyes of those whom it most concerns, to know them, and make them the guide of their conduct. Few medical practitioners in this country think of administering drugs to infants in health, with the view of keeping them well. This error is most commonly committed by half-educated parents and nurses, who have a foolish idea that, because medicine is good for curing disease, it must also be good for preventing its approach, and because the sick get it to make them well, the sound should have it to prevent them from becoming sick. Whether what has now been said of Scottish medical practitioners can be also said of the Irish, we know not. But we should suppose, that what is stated in the following remarks should be well known to those, who administer medicine to the healthy, or who evince any disposition to believe that medicine can do every thing.

"The importance which has, in many respects very justly, been attached to the condition of the digestive system in disease, has attracted much attention to its state in health; but, unhappily, the great majority of persons fall into the grievous fallacy of supposing that what is useful as a remedy, must also be useful as a preventative: and as the multitude has, in the case of the system in question, but one idea cognate with either remedy or preventative, viz. the idea of purging, the propriety of adopting a regular system of purgation, with healthy children, has been admitted almost without question. There cannot, however, be a more dangerous absurdity than such a system; and in proof of our opinion, we shall ask a single question, and bring forward one fact. What is the mode of action and effect of every aperient drug in the pharmacopœia? Is it not irritation, direct or indirect, of the intestinal mucous membrane, in a greater or lesser degree; and does it not produce, as a necessary effect, in every instance, a larger or smaller quantity of increased secretion? So much for our question. We presume that no person, but a devoted disciple of St John Long, will maintain that irritation producing increase of secretion, can ever be necessary in the *healthy* body. With respect to our fact—let any person with regular, or moderately regular bowels, take an aperient, and he will certainly find that after its immediate effects have passed away, a state of costiveness will remain. The tendency of nature to periodical movements, has been interrupted by the production of evacuations at irregular periods, and she requires some time to enable her to recur to the simplicity of her original design.

"We wish it to be distinctly understood, that what we are now advancing has no reference to the *use* of purgatives in disease, but solely to their *abuse* during health; and we are the more particular in endeavouring to convey our own views upon the subject, because we know that the impropriety of a needless resort to medicines of this kind, is not sufficiently thought of by medical men. We have, indeed, too often had occasion to lament over the display of drugs upon the mantel-piece of even a medical man's nursery, not to feel special interest in this matter. The least judicious would surely grieve, did he see an ointment of cantharides rubbed to a healthy skin with no other apparent intention than that it might be followed by a healing salve; yet, what less absurd, is portended by the accumulation, in such a situation as we have alluded to, of packets of aperient draughts, bundles of restraining powders, and bottles of carminative mixture—all designed for the same unlucky children. This is no imaginary description, we have but too often seen its original; yet, better and more merciful would it be, towards many of the victims, to destroy them in the birth, than by such a course, to provide for them the enduring miseries of an irritable intestinal mucous membrane."—Pp. 61, 62.

In the third chapter, on Mental and Moral Education, also by Dr Maunsell, that difficult subject is treated with skill and judgment. Our limits, however, permit us merely to make a general reference to it; and to repeat what we have more than once had occasion to say in the pages of this Journal, that, upon the due regulation of the affections and the government of the passions at this early period, depends all the happiness of the individual, and the prospect of his utility in society.

The fourth chapter, on the Peculiarities of Disease, by Dr Evanson, consists of three divisions; Etiology, or a view of the causes of Disease in Infancy; Diagnosis, or the means of distinguishing from each other the different disorders incident to the infantile period; and Prognosis, or the knowledge of the circumstances which are to regulate our judgment as to its termination in health, in death, or in other disorders.

The importance of accuracy and precision in diagnosis is very strongly impressed; and Dr Evanson properly directs the chief attention to the expression, the physiognomy, and the attitudes, presented by infants, in enabling the practitioner to judge of the presence, the nature, and the degree of disease. It is indeed impossible in infants who are unable to express their feelings otherwise than by cries, or the expression or attitude which they assume, to avail ourselves of the usual methods of procuring information which are employed in studying the diseases of adults; and hence the study of diagnosis in the diseases of infants requires a peculiar education, and, if not a certain degree of tact and acuteness, a quickness and precision of observation only to be acquired by long experience. This quickness and precision is so much the more requisite, as our prospect of successful treatment depends often mainly, if not altogether, upon the early recognition of the presence of disease, of its characteristic nature, and of the extent to which it has proceeded.

" The healthy child is cool, plump, and lively, the flesh firm, and movements free. It likes to be played with, and carried about. The abdomen is full and soft, and pressure upon it seems rather to please than annoy the child. It sleeps quietly on its side, with the limbs flexed, generally going to rest at the same hour; and awaking cheerfully, soon demands food. It is scarcely necessary to add, that the secretions are natural and regular. Not so with the child when ill: it is more or less peevish—dislikes being stirred, or even cries when handled; its hour of sleep is uncertain, and it rests ill, or awakes startled or crying. The skin is hot and dry, particularly of the hands, feet, and head. The flesh is soft or wasted. There is thirst, with unnatural appetite and depraved secretions.

" When a sick child is brought to us, or we visit it, our attention should first be directed to the expression of the countenance, then to the attitude, state of the limbs, and skin. In the child in particular, the expression of the countenance or peculiar character of certain of the features, indicates a morbid condition of particular organs; for at this age the expression of the countenance is an index of physical, rather than mental, operations; sensation more than thought being expressed thereby. We next proceed to examine more minutely into the several symptoms,—to determine the seat of the disorder, whether in the head, chest, or abdomen,—and its nature, or the particular lesion on which it depends.

" When the brows are knit, the eyes fixed and staring, or looking wild or vacant, our attention is at once directed to the head. We try whether it is unusually hot, the vessels full or in over-action, and examine the fontanelle, to see if it be depressed or in a state of distension. We now observe the attitude, and see whether the child's head hangs heavily on the nurse's arm, or rolls from side to side, sunk upon the pillow; and try whether the neck be stiff or retracted, which it occasionally is. We examine the limbs, to see whether they be rigid or relaxed,—lying motionless, or tossed about, and affected with spasm—in particular, we try whether the hands are clenched, and the thumb turned in, or toes bent. According as the upper or lower extremities are affected, one or both, an indication is given as to the seat and extent of the disease. The state of the pupil next demands investigation. It may be found contracted, and the child will start, as if frightened, or scream out, on being touched; or the pupil may be dilated and insensible to light,—the child being motionless or unconscious. We shall hear that the child has had starting in its sleep, grinding of the teeth, or movements of the lips; and that when awakened, it started up affrighted, or screamed out; the bowels are obstinately costive, or the evacuations very foul and dark-coloured. The hands are usually hot, but the feet cold; and one cheek is often deeply flushed. No doubt can now exist that the head is the seat of disease; but what that particular disease may be, and whether a primary or secondary affection, is to be determined by symptoms hereafter to be detailed in their proper place.

"Occasionally we see a child lying completely prostrate, the limbs being relaxed and motionless, the belly sunk, and the eyes fixed, with the lids half-closed; we are told, however, that it is now much better than it was, as it had been affected with convulsions; and no suspicion seems to exist that it is in the last stage of cerebral disease,—the attendants actually congratulating themselves on its quietness. It is not uncommon to find one or more of the extremities quite paralytic in the young infant, without any notice having been taken of the circumstance.

"There is another expression of countenance, very different from that last described, but which denotes much suffering. The lips are retracted, or drawn, so as to show the teeth or gums; the countenance is pale, or sallow and sunk; the child seems to dread motion, lies on its back with the knees bent or drawn up, and is pained by pressure on the abdomen. Here neither the brow is knit, nor the pupil of the eye affected. The skin is very hot, shrivelled, and harsh; there is much wasting, great thirst, particularly for cold water, and a distinct diurnal exacerbation. The tongue is loaded, or dry, red, and pointed; aphthæ may be present; and diarrhœa constantly attends,—the stools being generally thin, green or dark brown, and fetid. The abdomen is here obviously the seat of disease; and such a state of things often occurs at the period of weaning, as the state before depicted does during dentition.

"In the autumnal season, we often see an infant lying sunk in the nurse's arms, the face pale, the features sharp, and the countenance betokening distress; it may be that the eyes are sunk, and the skin around them, as also the lips, dark-coloured. On our first query, we will be told that the child has vomiting and purging; but a look must be sufficient to tell the experienced eye that this is a case of infantile cholera."—Pp. 91-93.

Prognosis is of not less importance in infantile diseases; since not unfrequently it is the hope that the child may recover, that induces the practitioner to persevere in his cares, to proceed with treatment, to modify it, or to abandon it; and in some instances recovery takes place under most unpromising and inauspicious circumstances. It was the saying of one who was no mean judge, viz. Dr John Gregory, that " a physician ought scarce ever to despair of a child's life, while it continues to breathe."* There is, however, a foolish practice, originating apparently in the vanity of aspiring at the character of superior skill, of declaring that such a child must die, and that recovery is out of the question. No one can deny that disorders of this kind are not unfrequent, and too often it is more easy to prognosticate a fatal than a favourable termination. But it is of the utmost moment in infantile diseases, the indications of most of which are extremely obscure, never altogether to abandon hope while vital action lingers in the veins; and he who is best acquainted with the resources of the system will be least inclined to do so.

" Generally speaking, we may say that, however severely a child may be affected in the first stage of an acute disorder, or to whatever extremity it may be reduced in the progress of a protracted one, it will be wise not absolutely to give up hope, however guarded our prognosis may be. Few who had not witnessed it, would credit the immediate benefit to be derived from the employment of sufficiently energetic means in the early stages of the severer forms of infantile disease, or the apparently moribund state from which a child may be restored by judicious management. One evil arises from early despairing of a child, which the practitioner should keep in mind and guard against. The attendants, who are always ready to give up administering medicines, which they often look on as only a source of annoyance, will from that time do nothing; and thus children are allowed to perish for want of proper perseverance in the administration of the remedies prescribed."—Pp. 97, 98.

The fifth chapter, on Infantile Therapeutics, by the same author, presents a view of the medicines and classes of medicines, and the remedial agents in general, most usually employed and chiefly required in the treatment of the diseases of the infantile period. The chapter constitutes a sort of system of Materia Medica, and a Pharmacopœia, of which it is not easy to give any detailed account. The author considers the different remedies, and the diseased states in which their use is indicated, in the following order:—Mercury, sedatives, including the internal administration and the external application of opiates, alkalis and alkaline earths, carminatives, stimulants, tonics, the antiphlogistic plan of treatment, blood-letting, blisters, emetics, expectorants, diaphoretics, refrigerants, baths, and purgatives. From this list, it is evident, that these remedial agents are not considered according to any principle of order or method. It is therefore not so objectionable by the badness of the arrangement as by the total want of all arrangement. As, however, the mode appears to answer the views of the authors indifferently well, it can scarcely be viewed as a fit subject for critical examination. Under the head of diaphoretics we were rather astonished to see nitre placed, since that salt is more a diuretic and refrigerant, and, according to our ideas, would have found a more appropriate place in that situation. Conversely, baths, both hot and cold, to which here a separate place is assigned, have always appeared to us to have a diaphoretic effect, especially the former, and sometimes a diaphoretic and diuretic effect, and, therefore, to be with advantage referred to the head of the former.

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* Comparative View of the State and Faculties of Man, &c. Section I. p. xxix. Edin. 1788.

In the sixth chapter, on the Accidents and Diseases occurring at Birth or shortly afterwards, by Dr Maunsell, is given a short history of each of the following species of malformation or disorder. The various forms of Still-birth, Imperforate Anus, Imperforate Vagina, and Imperforate Penis, Spina Bifida, unusually Short *Frenum Linguæ*, Hare-Lip, Deformed Feet, *Nævi Materni*, Ruptures, Injuries received during Birth, *Morbus Cæruleus*, *Cyanosis*, or Blue Disease, *Trismus Nascentium*, Infantile *Erysipelas*, Abscesses, Induration of the Cellular Tissue, Jaundice, Purulent Ophthalmia, Red Gum *(Strophulus intertinctus,)* and Swelling of the Breasts.

The seventh chapter, on Dentition, gives a view of the disorders occasionally accompanying the eruption of the milk teeth, and the remedial measures most suitable for alleviating their severity.

In the eighth chapter, on the Diseases of the Digestive Organs, by Dr Evanson, we find the author entering on the consideration of the multiplied disorders most common to the periods of infancy and childhood, which are treated in the following order. After some general observations on the pathological relations of the gastro-intestinal mucous membrane, the author, under the head of affections of the Mouth, Pharynx, and Œsophagus, gives accounts of *Stomatitis*, or inflammation of the oral mucous membrane, Aphthous Ulceration, White Thrush or Millet, Diphtheral inflammation, Gangrene, *Cancrum Oris*, and Mumps.

Diphtheral inflammation, which is perhaps the most important, because the most dangerous, is well though briefly described. The following extract will communicate some idea of the mode in which the practitioner is to distinguish this from similar disorders, to prognosticate its issue, and to think concerning its nature.

" *Diagnosis and Prognosis.*—The affections with which diphtherite is most likely to be confounded are, aphthous ulceration existing in the pharynx, or the gangrenous sore throat. It is not uncommon to find aphthous ulceration in the situation described, denominated diphtherite; for with some every spreading ulceration at the back of the throat goes by this name; yet, the distinction is at once obvious and easy, the ulcerated state of the mucous membrane, on removing the aphthous incrustation, distinguishing it from the merely inflamed condition of the same surface in diphtherite. Equally decisive is the distinction between the malignant form of the complaint and gangrene of the throat, in which the mucous membrane is found in a state of slough or sphacelus, while in diphtherite this membrane, although it may be dark-red or even livid in colour, is still found in a state of integrity on the removal of the putrid membrane.

" Our prognosis in this disease must be always guarded, and in particular we must take care not to be misled by the absence of any uneasiness in the throat in the first instance, but when diphtherite prevails, be particular in examining the fauces, the moment a child sickens or appears ill; for in some instances the disease will be found to be decidedly established before any complaint has been made of the throat, and the worst form of the disease may commence in this insidious manner. When the child is healthy, however, and the disease of the acute kind, but not severe in degree, there is no particular cause for alarm, for the thin white and smooth exudation does not in general show a disposition to spread beyond the pharynx, and may be seen not uncommonly as an accompaniment of scarlatina or measles. Even in the still severer form, where the lymph is thrown out in masses, as already described, there is not a tendency to spread to the larynx, though the deposition at times appears on the œsophagus. In very acute cases of the most healthy form, however, the false membrane will spread into the larynx, if not early arrested; and in some instances its formation seems to occur almost simultaneously in the air-passages and on the pharynx, the croupy symptoms appearing to co-exist with the appearance of the lymph on the fauces: such a case we need hardly say must be almost necessarily fatal. In the malignant form of diphtherite, we have a double source of danger, a fever of the typhoid type being added to the almost certain affection of the trachea, for unless arrested in its progress, the false membrane uniformly descends into the air-passages in this form of the complaint, which constitutes one of the most fatal and fearful diseases to which the child is subject. We are happily, however, not left without resources even in this formidable complaint, and although our prognosis must always be unfavourable, the prompt application of remedial means exercises a decided control over the disease. Even after the air passages have become engaged, recovery seems in some cases to have been effected.

" *Pathology.*—Having noticed so fully the nature of the deposition in this disease, and the condition of the mucous membrane connected therewith, it only remains to observe, that on post mortem examination the false membrane has been found extensively deposited, covering not only the parts visible to the eye during life, but extending in some instances along the œsophagus, even to the cardiac orifice of the stomach; and almost uniformly occupying the trachea, to a greater or less extent, the bronchial tubes even not being free from its presence. The membrane is a deposit of true lymph, though varying in appearance and consistency, according to the nature of the case. Hence this disease has been looked upon as identical with croup by Bretonneau; and Guersent seconds him in this opinion, which is however obviously to confound two distinct diseases.—Pp. 211, 212.

This represents with sufficient detail the effects of the disorder. The danger and fatality are justly represented to depend on the extension of the inflammatory action into the air-passages, especially the larynx and bronchial tubes, and the consequent formation of the plastic coating over the inflamed membrane.

In treatment, the great object is to oppose the spreading of the inflammation, and for this purpose Dr Maunsell recommends, as most authors have done, a strong solution of nitrate of silver, (twenty grains to the ounce,) or a saturated solution of sulphate of copper, to be applied to the pharyngeal mucous membrane, by means of a camel hair pencil. In the formidable forms and the advanced stage of the disease, he recommends preferably the application of hydrochloric acid, undiluted, or mixed with one or two parts of honey,—by means of a piece of sponge attached to the end of a probang.

Dr Evanson then proceeds to the consideration of the affections of the stomach and bowels,—embracing Indigestion, Weaning-Brash, Cholera, different forms of Diarrhœa, and the Inflammatory diseases of the alimentary canal. Next follow Mesenteric Disease, then Worms, and lastly, Remittent Fever.

On the subject of Worms, Dr Evanson properly remarks, that the symptoms believed and represented in books to indicate their existence are not pathognomonic. It is even known that worms may exist in the intestinal tube without giving rise to any particular class of symptoms, and certainly without producing those ascribed to their presence; and it is also well known that the symptoms supposed to proceed from the presence of worms most usually depend on irritation of the intestinal mucous and muscular tissues. The only sure mode of determining the presence of these parasitical animals is by perceiving that they are discharged dead or alive with the excrements.

In the ninth chapter, under the head of Diseases of the Respiratory Organs, Dr Maunsell considers successively the phenomena of the following diseases, as they appear in infancy and childhood; Catarrh, *Bronchitis*, *Pneumonia*, Pleurisy, Croup, *Laryngitis*, and other disorders of the Larynx, the symptoms caused by foreign bodies in the Larynx and Windpipe, Crowing

Inspiration, and Hooping-Cough. The descriptions of these several disorders are well suited to the general object of the work.

In the tenth chapter, on the Diseases of the Cerebral System, Dr Evanson gives a view, first, of the various dynamic or functional disorders, as he terms them, and then of the inflammatory affections. Under the head of Functional Diseases are enumerated the symptoms of cerebral Irritation or Erethism of the late Dr Whitlock Nicol, Wakefulness, Somnolency, or preternatural Drowsiness, Congestion, the Hydrencephaloid Disease, and Convulsions.

Somnolency or preternatural drowsiness is an occasional symptom of sympathetic suffering in the brain, from an overloaded state of the stomach, disordered, especially costive, state of the bowels, and sometimes the presence of worms. The difficulty in one of these cases is, that the costive state of the bowels is often a torpid or inert condition proceeding from the same state of the cerebral circulation which gives rise to the preternatural somnolence. This state we have witnessed in children somewhat grown up, and found it disappear only after the repeated use of eccoprotic medicines, shaving the scalp, and the application of cold to the head.

Under the name of Hydrencephaloid Disease, Dr Evanson notices the peculiar state of feebleness, drowsiness, and occasional delirium, with dilated pupil, half-closed eyes, feeble, rapid, but variable pulse, with coldness of the extremities, which sometimes comes on and imitates the symptoms of meningeal inflammation, but depends rather on great emptiness of the vessels, and indeed may be regarded as an incomplete or continued form of syncope. The best treatment is the moderate use of stimulants, opiates, and the warm bath. "In this disease we must be careful to avoid putting cold to the head, or raising the child suddenly up and keeping it in an erect position. By attention to these particulars, children may be saved, who are said to be dying from hydrocephalus, but who must have irretrievably perished had antiphlogistic measures been employed."

The account of Water in the Brain, though not altogether so detailed as that disease has been made by various recent writers, is sufficiently so for all practical purposes. In speaking of the treatment, the author properly impresses the necessity of treating the disease as one eminently inflammatory. But, according to our own experience, he is much too indulgent to mercury, iodine, and foxglove, which are not only in themselves useless, but often injurious, by leading the practitioner to neglect the use of the more effectual measures of general and local blood-letting and the use of cathartics.

Of the chapter on the Eruptive Diseases we say nothing, save that it is a good summary of the most important facts known on each; and that the observations on the employment of blood-letting in scarlet fever are not in the proper spirit for a didactic work. The author recommends Huxham's advice to peruse the sixth commandment, (p. 390.) It is best for all of us never to forget either the letter or the spirit of this important precept. But we further recommend the authors, and practitioners in general, to peruse the paper of Mr Sandwith in the fortieth, and that of Mr Dewar in the forty-fourth, volume of this Journal.

The chapter on Vaccination places the true merits of that practice in the most correct point of view.

In the thirteenth chapter, on Constitutional Diseases, by Dr Maunsell, we find under the title of *Pemphigus Gangrenosus* both the name and description given, in the Dublin Medical and Physical Essays, (Volume i. p. 146,) by Dr Stokes in 1807, of that peculiar disease called burnt holes by the Irish peasantry. Dr Tuomy of Dublin, in his useful treatise on the principal diseases of that city, reproduced with acknowledgement, however, the description of Dr Stokes, and added a few observations confirming the general accuracy of Dr Stokes. The present author has also given the description of Dr Stokes, which is indeed so accurate, that it is difficult either to enlarge, to abridge, or to modify it without injury.

As it is a disorder most known to the peasantry, it has received from them different appellations in different counties. In the counties of Dublin and Wicklow it is called the White Blisters; in the counties of Down, Antrim, Derry, and Monaghan it is named the Eating Hive. In the counties of Waterford and Tipperary it is known as the Burnt Holes. Dr Stokes first applied to it the name of *Pemphigus Gangrænosus ;* and the name was adopted first by Dr Tuomy, and by the present authors.

The following is the description of Dr Stokes :

" ' The approach of this disorder is sometimes, though rarely, denoted by a livid suffusion, like that of erysipelas, slightly elevated. It more frequently happens, however, that the complaint comes on in perfect health. One or more vesicles appear, mostly larger than the best distinct small-pox : these increase for two or three days, burst, and discharge a thin fluid, having a disagreeable smell, limpid in most cases, sometimes whitish, and sometimes yellowish ; the latter less dangerous : usually, the weaker the child's constitution is, the thinner is the matter. Before or after breaking, the vesicles run together ; the sore becomes painful, with loss of substance, and a thin, fetid, ichorous discharge ; the edges of the ulcer are undermined, and it spreads quickly.' "—P. 470.

To this the present author adds the following observations :

" We have sometimes observed the sore to put on a very peculiar appearance, being clean, but perfectly destitute of granulations, without any surrounding inflammation, and seeming exactly as if a portion had been scooped out of the sound skin with a sharp gouge.

The surface of the sore is, in other cases, livid and sloughy, true gangrene actually taking place in it."—P. 471.

Dr Stokes then proceeds in his description.

"The most usual seats of the disease are, behind the ears, sometimes on the hands or feet, on the private parts, (seldom on the arm-pit,) the breast, folds of the thighs, lower belly, on the inside of the mouth or lips. If the sores are behind the ears, they destroy the connection of the posterior cartilage with the cranium; they spread to the meatus auditorius; to the eyes, the sight of which seemed, in a few cases, to have been destroyed one or two days before death; and they sometimes extend to the vertex.

"The constitutional disturbance, that accompanies this disease, seems principally the effect of irritation. When the vesicles burst, the child begins to grow peevish and fretful, pale, loses its appetite, and the flesh becomes remarkably flabby. The periods of the disorder are not very regular; but it often happens, about the eighth day, that the pulse sinks, the lividity spreads over the whole sore, the fœtor and discharge increase greatly. The smell is so strong as often to be perceivable at a distance from the bed. The discharge in one case, where the ulcers affected the arm-pits and breasts, was such that the linen was completely loaded several times a day.

"Death takes place about the tenth or twelfth day, often preceded by convulsions, sometimes by extreme debility. Patients are apt to relapse soon after the sores are skinned over.

"The causes of this malady are rather obscure. It seems exclusively confined to children. Dr M'Donnel saw twenty cases before the year 1795; all the patients were under four years old. Dr Spear observed, that it was confined to children from the age of three months to that of five years; but it has been observed, near Dublin, in children of nine years old. It attacks the finest children in preference; the children of the poor more frequently than those of the affluent; and those who live in damp situations seem more peculiarly subject to it than others. The disease is more prevalent in summer than in winter. It appears to be infectious, though obscurely so, in general; but, in the year 1800, Dr Spear observed it to spread epidemically."

"It has been alleged, by empirical practitioners in this disease, that, after the blackness had covered the whole sore, death was certain: but I have observed the blackness to go off, although it had spread over the whole surface of the sores. When this appearance abates, livid streaks generally remain for a day or two. When a favourable change is effected, in bad cases, the diminution of the fœtor and discharge were the first signs of the abatement of the malady; appetite was afterwards restored."—P. 472.

As this disease very generally proceeded to the fatal termination, it was important to ascertain if there was any method of management, general or local, under which recovery might appear to be accomplished. In this difficulty, Dr Stokes found that the peasantry themselves had been long in the habit of employing, with considerable advantage, as local applications, various compositions, all of which agreed in the circumstance of containing some green vegetable matter. He at length made choice of the leaves of the *Scrophularia nodosa.*

" As I had also many other recipes for making a green vegetable ointment, and had good grounds to suppose, that several different compositions of this kind were used with success, I determined to make an ointment of a single vegetable; and, in selecting that vegetable, I was directed by its occurring in many recipes of this kind, and having itself a character among the common people, as a useful application to obstinate ulcers. Upon this ground, I fixed on the *Scrophularia nodosa,* called, in the north of Ireland, rose-noble; in the south, phoghram, pronounced phoram; in England, great figwort. This simple ointment, I hope, is as powerful as the compound, although my success with it has not been uniform; but the failures, which have been very few in comparison of the successful cases, have been those not under my own eye, or very far advanced: and I have reason to know, that no person is so successful as to be sure of rescuing the patient in all stages.

" The following method is nearly what I have pursued for several years past, and what I advise. When the parts adjoining the sores are swelled, and strongly suffused with a dusky redness, or if the sores have been previously dressed by any dry powder, I apply a poultice of porter and oat-meal. The carrot poultice, in fermentation, if it can be procured without any delay, would perhaps be useful. After about eight hours, the poultice should be removed, and the parts affected very gently wiped with a roll of lint or soft rag; then the Scrophularia ointment should be applied. It should be as highly saturated with green vegetable matter as possible. For this purpose, the plant should be taken fresh, the smaller leaves selected, and stewed a considerable time with as small a quantity of unsalted butter as will be sufficient to prevent the leaves from being scorched. If well prepared, it is of a full grass green colour; but after keeping it becomes the colour of box leaves, especially at the surface; yet I apprehend it preserves its efficacy, in a great degree, for many months. When applied, it should be melted, and suffered to cool to the consistence of honey; it should be applied, by a soft feather, with the utmost gentleness, to the whole surface of the sore. Through the whole of the treatment, the greatest gentleness should be used. If the ear is strongly drawn open, the parts affected are made to bleed, which produces many inconveniences, and retards the progress of the cure. After smearing the ulcer with the ointment, it should be dressed with the same ointment, with the addition of one-eighth part of wax. This last ointment should be spread on lint folded to the dry side, and cut so as to fit behind the ear; the whole should be secured by a broad bandage, drawn under the chin, and fastened over the top of the head. This dressing, in very severe cases, should be repeated every fourth or sixth hour; but, as the fœtor abates, larger intervals may be allowed.

Hairs should be completely removed from the neighbourhood of the sore. I believe it to be necessary that the child's bowels should be kept open. I also direct the internal use of yeast, which I am of opinion is of service, but cannot decidedly prove it to be so.

"Such is the treatment I propose for this disease, selected from such parts of the traditional method of cure as seem to have been most successful, with a very little addition. I do not pretend that this treatment will always succeed; but perhaps of four such cases as may occur to any practitioner, not excluding the most hopeless, it will succeed in three on an average: and if we could accurately ascertain the mortality which takes place when other methods are used, we should consider this proportion of success as very satisfactory. I have had a variety of opportunities of concluding, with a high probability, that the treatment I recommend is superior to any of the following applications. The carrot poultice, although this, I doubt not, has been of use in few cases; preparations of mercury, of lead, of zinc; powder of bark, of starch; washing with brine, with soap and water. I have named these in the order in which they are more and more objectionable. The first I believe to have been useful; although I have pretty well ascertained, not so useful as the green vegetable ointment: the last I have some reason to believe to have been pernicious.

"The utility of the Scrophularia ointment does not stand on my testimony alone. I have annually mentioned, at lecture, the state of my observations on this disease, and I have received favourable accounts of the use of it from several friends; some engaged in the profession, and some not. Mr Creighton, surgeon to the Foundling-Hospital, particularly authorizes me to state, that he used it for upwards of a year with uniform advantage.

"I do not pretend to have proved, that the Scrophularia has a specific effect on this disease; possibly it is only useful, by supplying the green vegetable matter; but as I and some of my friends have had a success more uniform, by means of this ointment, in a disease in which our failure was almost uniform before, I shall continue to prefer it to other vegetable matters until the subject is better understood."

In consequence of the *Scrofularia nodosa* being thus employed beneficially, in the form of an ointment, in the practice both of Dr Stokes and his friends, and the evidence being confirmed by Dr Tuomy, a formula for its preparation was introduced into the Dublin Pharmacopœia of 1826. This is the history of the introduction of the substance into the *Materia Chirurgica* of the King and Queen's College; and, however unimportant it may appear, we think that the history of the matter deserves to be better known, than we believe it is, to many of our brethren on this side of St George's Channel. We have heard several English and Scottish surgeons indulge in a sarcastic sneer at the confidence which appears to be reposed by Irish surgeons in the virtues of the *Scrophula-*

ria ointment; and even our beloved and lamented friend, the late Dr Duncan, who perhaps was not much less sceptical in allowing the alleged virtues of many vegetable substances, than his predecessor Dr Cullen, allowed himself to say, "that, without presuming to deny its utility, he had to confess his ignorance of its virtues."* This is, it must be confessed, the great point to be ascertained; and the only mode in which it can be effected is by subjecting the remedy to trial, if not in the treatment of the same disease, at least in that of analogous inflammatory and gangrenous disorders. This it would be easy to do, as the plant is indigenous in many parts in Scotland, and grows abundantly in the neighbourhood of Salisbury Craigs, near this city.

This is so much the more requisite from the fact, that the present authors altogether concur in the excellence of the method of treatment pursued and recommended by Dr Stokes, and admit that they have found considerable advantage in the employment of the figwort (*Scrophularia*) ointment. We must not fail to notice, however, that they attach great importance to the adoption of very careful general treatment.

> "Change of locality, and free ventilation of the patient's apartment, should, if possible, be effected at the very commencement. The bowels should be regulated by alterative aperients, which commonly bring away dark-coloured and offensive stools. The diet should be nutritious, but not stimulating; and if the child be suckling, a fresh and healthy nurse should be procured. As soon as the bowels have been set to rights, we commonly administer quinine; and if there be no diarrhœa, we may combine it with one of the mineral acids."—P. 492.

With regard to the nosological character of this disorder, we entertain some doubts, notwithstanding the high authority of Dr Stokes. We have met, in this country, with only two cases which, so far as we can compare them with the general description given by Dr Stokes, seemed to be referable to this head. In both it affected the fore-skin and the skin of the penis to its connection with the pubis; and in one, which terminated fatally, it also affected both ears. In neither of these cases, however, did we recognize the vesicles which are characteristic of the appearance of *herpes*, but rather appeared in the form of a hot erythematic redness, with hardness and great pain of the part. In both these cases, indeed, the disease, though occurring in male infants, bore in its appearance a close resemblance to that described by Dr Percival, and afterwards by Mr Kinder Wood; and we perceive that Dr Maunsell regards the latter disease as in several circumstances closely allied to the *pemphigus infantilis* of Dr Stokes.

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* Edinburgh Dispensatory, twelfth edition. Edin. 1830.

In the fatal case seen by the author of this notice, serous fluid was found in the subarachnoid tissue and within the lateral ventricles of the brain, considerable portions of both lungs were in a state of circumscribed dark-red induration, or hemoptysical engorgement, numerous intus-susceptions were found in the course of the ileum, and opposite to each intus-susception were one or more of the Peyerian or aggregated patches of follicles enlarged, prominent, and tumefied.

In conclusion, the practitioner will find in this work a convenient and safe guide in enabling him to understand the nature, and conduct successfully the treatment of the various disorders incident to the age of infancy and childhood. The work, indeed, is not remarkable for novelty, as in most of the diseases considered what was obscure or uncertain, is not rendered more clear. With this, however, it would be unreasonable to find fault. The work is distinguished for much sound sense and correct judgment; and we think one of its leading recommendations is, that it will teach practitioners generally, and the junior in particular, the mode of observing infantile disorders, and recognizing their existence with much more promptitude and celerity than heretofore, and consequently, insure in curable cases a much more rational and efficient treatment.

ART. III.—*Medico-Chirurgical Transactions, published by the Royal Medical and Chirurgical Society of London.* Vol. xxi. London, 1838.

It is not very long since we had occasion to bring before the notice of our readers, the 20th volume of the Medico-Chirurgical Transactions; and we have now much pleasure in directing their attention to the 21st, as a creditable continuation of the labours of the very respectable association from which it emanates. The volume consists of twenty-four articles, of which eight principally relate to subjects in medicine, eight to those connected with surgery, two to chemistry, one to physiology, and five to pathology. It is entitled the *third volume* of a *second series*, intended to begin with the 19th volume of the Society's Transactions, (which was the first that was published after the granting of the charter,) and to be continued in connection with the enumeration carried on from the commencement of the work. We have considerable doubts as to the expediency and propriety of such a change. It is likely to affect, in an important degree, facility and precision of reference, by introducing a double, or interchangeable designation into the Society's volumes; while it will lessen the prominence of that honourable position in the scientific world, which the Society owes, in a certain degree, to the well-known and creditable extent of its publications. It likewise appears to us, that, if the present alteration is a proper one, introduced, as it has been, without the occurrence, on the part of the Society, of any change in its plan or system of management, similar alterations must take place, after certain arbitrary periods of time, or extents of publication. Viewing, therefore, the Medico-Chirurgiral Transactions as an important and permanent work of reference for this and other countries, we consider that the designation of its volumes which has hitherto been adopted, in continued numerical sequence, is simple, intelligible, and not liable to mislead. If this enumeration is to be kept up, the new one is unnecessary; and we would therefore, with every feeling of respect and regard, take the liberty of suggesting to the Council and to the Society, a reconsideration of the subject, while it is yet in time. We are aware that in the *trade*, hopes of promoting an increased sale, sometimes give rise to changes in the mode of getting up works, which would not otherwise be adopted. But supposing that such hopes were likely to be realized in the present instance, (which, from the ready diffusion, by means of the weekly, monthly, and quarterly press of the information communicated at the meetings of societies, we think but little likely to be the case,) we should still consider the justification as imperfect and unjustified, and the measure itself inconvenient and unnecessary.

We should venture to add a few words more on introductory matter. The very respectable printer of the Society's volumes, enamoured of his lately acquired dignity of printer to Her Majesty, has thought fit to place the Queen's arms over his name, in the reverse of the title-page; so that the modest, unassuming notice which the law requires printers to give of their share of a work, is now to contend with the title-page, for the attention of the reader. If such a decoration is admissible at all, we pray it to be at the conclusion of the volume, leaving to the authors and publishers undisturbed possession, as heretofore, of the title-page, both obverse and reverse. We believe it to be contrary to all usage for an engraving to appear on both sides of the same leaf.

In proceeding to the consideration of the contents of this volume, the first of the medical papers which presents itself to our notice, consists of a *Note on the Prevalence of Calculous Diseases, &c.;* by A. Copland Hutchinson, F. R. S. London and Edinburgh.

The former publications of the author on the comparative unfrequency of calculous diseases among sea-faring people are well known throughout the medical profession; and the object of the present communication is to offer some further evidence on the same subject, and to communicate some additional information in relation to calculous diseases generally. By a letter which is given from Sir William Burnett, it appears, that since the period of the last communication made by the author to the Society in 1830, only one case of calculus, and that renal, had occurred in the various naval hospitals at home and abroad, though the average number of seamen and marines annually voted by Parliament from that time to the present has been 30,000, including 2000 boys.

The author quotes an interesting passage from Aretæus, in which it is stated, that diet, (meaning by δίαιτα, of course, the proper regulation of diet,) and anointing, *and* " *sailing, and passing one's life at sea,*—(καὶ πλοῦς, καὶ ἡ ἐν θαλάσσῃ βιοτὴ) all these are remedial in diseases of the kidneys." From this passage the author infers, that the only difference between him and Aretæus is, that the latter recommends " a sea-faring life, or voyaging, as a *cure* for stone and disease of the kidneys, while the statements of the former have gone to prove that these diseases do not commence or originate in a seafaring person."

Aretæus, the author informs us, is singularly mentioned by Priestley, as having flourished 300 years before the Christian era, instead of in the first century subsequent to it.

In Mr Hutchison's second paper in the Medico-Chirurgical Transactions, he was disposed to regard the occurrence of calculous diseases in Scotland as much more frequent than had previously been considered to be the case; but from the opportunities which he has recently had, of much personal inquiry on the subject, he thinks that he has still much underrated the frequency of its occurrence in one part of the island.

With regard to the treatment of calculous diseases, the author is of opinion, that " pure air,—a lax state of the bowels,—iodine used internally in proper doses, and externally over the region of the kidnies,—the use of swings, either in a garden or elsewhere,—active bodily exercise,—warm clothing, such as flannel-dresses worn next the skin, and a very sparing use of vegetables," are " the remedial and preventive measures indicated in such cases;" and " that one or more, or all of these, according to circumstances, may be advantageous, when sea-voyaging and a sea-life are impracticable."

The second medical paper is *on a Peculiar Symptom occurring in some cases of Enlarged Liver;* by John G. Malcolmson, Esq. Surgeon in the Madras Establishment.—In a case of severe hepatic disease, which was actively treated by general and local bleeding and mercurials, a tumefaction soon presented itself in the right hypochondrium, extending towards the umbilicus, and into the left side. In a few weeks fluctuation and tenderness exhibited themselves in it; but though the tumour showed a tendency to point at its upper part, it made no progress, and the patient was in every respect under very unfavourable circumstances.

At length he was "suddenly seized with acute pain over a small space of the anterior part of the left side of the chest, between the sixth and seventh ribs. There is a very distinct, sharp vibration communicated to the hand applied to the spot, with an emphysematous feel between the ribs, but attended with a loud crepitous bleating, distinctly heard without the stethoscope. Some mucous rattle in the throat, and he expectorates a little thick adhesive mucus. The respiration is hurried; he is exceedingly anxious, and is forced to sit up in bed. Pulse very quick and weak. Apex of the heart seems to be thrust up a little higher than usual."—P. 95.

It was considered as improper to open the tumour; but an incision was made upon it, for the purpose of diminishing the thickness of the parts, and assisting the progress of the matter to the surface. The removal of the tension, by means of the operation, gave the patient some ease; but he shortly afterwards died suddenly.

On examining the body, a large abscess was discovered in the anterior and superior part of the right lobe of the liver, but without any adhesion to the parietes of the abdomen. The liver was much enlarged; was rather soft; had several abscesses in its substance; and strongly adhered to the diaphragm. There was much reddish serum in the right cavity of the pleura, with flakes of coagulable lymph floating in it, and recent adhesions between the two pleuræ; the lung, though compressed, being healthy, and the adhesions easily separable. In the left side of the chest, "there was a slight recent adhesion of the thin margin of the left lung to the sixth and seventh ribs, at the spot where the sound was heard during life: the lung was well traversed and healthy, with the exception of an enlargement of some of the cells where it adhered. It was seen that the edge of the lung had been pushed against the side by the diaphragm, forced upwards by the enlarged liver. The heart and great vessels were healthy."—P. 98.

From the result of this examination, the author had no doubt that the peculiar symptom "of a loud sound, partaking of the character of a bleating, and of an ordinary respiratory murmur, but audible at some distance by the byestander, and accompanied with a strong vibration of the part of the chest from which it proceeded, was caused by the thin edge of the lung being compressed against the costal pleura by the enlarged liver."

Mr Malcolmson states, that he has found this diagnostic symptom very useful in many cases which have come under his cognizance, and for which he could not have been able to account, till informed on the subject by the result of the present dissection. He considers the use of percussion and of the stethoscope as more valuable in detecting enlargement of the liver than examination below the margin of the ribs, inasmuch as they afford the means of discovering that encroachment on the cavity of the chest, to which such enlargement, when it takes place either from congestion or chronic inflammation of the right lobe, often gives rise.

We have next a communication entitled *Nervous affections peculiar to young women, causing contraction of the muscles of the extremities, accompanied by increase, diminution, or absence of Sensation or Motion;* by John Wilson, M. D. Physician to the Middlesex Hospital.—This communication is principally composed of cases, ten in number, taken without selection from the author's case-book. The phenomena are many of them such as occur under some one or other of the forms of nervous diseases and hysteria; and in addition to the circumstances mentioned in the title of the paper, some of the cases were attended with pains in the region of the liver, and in the back, stretching down the lower limbs; with pain in the head, with convulsive affections variously modified, and with well-marked appearances of paralysis, generally assuming a hemiplegic form.

The treatment employed, generally, consisted in brisk purgatives; in local blood-letting; in the introduction of needles into the extremities, or the application of moxa; and the employment of carbonate of iron and douches of cold water.

The cause of the lameness which so often exhibited itself was frequently attributed to some accident, as a fall, knock, or sprain, occurring about the time of the lameness being first noticed; but the author is of opinion, that the patient is very apt to deceive herself on this point, and may become lame, without having received any external injury of the parts affected.

" In the cases related, the women were all young, generally in good health, and of strong constitutions: all of them were single, and many of them subject to violent hysterical fits. In the four most obstinate cases, the functions of the uterus were regular; in others, the bowels were confined, and in four of them obstinately.

" Again, at times, some were seized with sudden and apparently alarming symptoms, as repeated hæmoptysis, most violent convulsive fits; others, on the contrary, had perfect paralysis of both sensation and motion; another lay motionless and speechless for three days: yet these symptoms only suspended, and never altered the treatment."—P. 123.

A report of a Case of Secondary Measles, with observations; by Joseph Moore, M. D.—This was the case of a healthy female child of 22 months, who, in the latter part of May 1836, was attacked with measles, from which she had entirely recovered by the 10th of June. All the other children of the family, (six in number) had the disease in succession, and on the 31st of July following, the child first attacked had an accession of fever, which was succeeded by a rubeolous eruption at the usual period. The symptoms were in an aggravated form; but by the 9th of the following month (August) the child was convalescent, though a dusky hue long rested on the parts which had been occupied by the eruption.

The author adverts to the doubts which had been long, and are still, to a certain degree entertained, relative to the second occurrence of measles; but he refers to the authority of Dr Baillie's paper in the 3d volume of the Transactions of a Society for Improving Medical and Chirurgical Knowledge, as one of the earliest confirmations of the fact, to which he adds the evidence of Home, Guersent, and Rayer, in conjunction with that of the present case.

Though there has been much difficulty experienced relative to the establishment of a definite diagnosis between measles and scarlatina, the author states, that he has never seen an example of measles, " in which congestion and redness of the conjunctivæ, especially of the lower eyelids, has not existed, and that too, occasionally, with very slight epiphora." He is also of opinion, that the less may be the lacrymal secretion which prevails, the more marked is the palpebral turgescence. " Cuticular elevation, or prominence on the parts occupied by the eruption," as contradistinguished from the " tumefaction of the cutaneous surface," prevalent in small-pox and scarlatina, is likewise to be seen in numerous instances of measles; while in the fauces and *velum pendulum*, the redness is more diffused than in scarlatina, is unaccompanied with swelling of the parts, and does not impede deglutition.

Dr Moore is of opinion, that many systematic authors have been much deceived in founding nosological distinctions upon slight shades of difference, and erecting a few anomalous cases into new varieties. The ***Rubeola sine catarrho*** of Willan; the ***Rubeola incocta*** of Good; the ***Rubeola sine eruptione*** of De Haen, Burserius, and others, are recorded by the author as very equivocal forms of disease.

The next article is *a report of twenty cases of Malignant Cholera that occurred in the Seamen's Hospital Ship, Dreadnought, between the* 8th *and* 28th *of October* 1837; by George Budd, M. B. F. R. S. Physician, and George Busk, Esq. Surgeon to the Dreadnought.—This communication contains a well arranged and interesting account of the various phenomena attendant on the attack of cholera which it describes; together with the morbid appearances which existed in the cases which terminated fatally; and a summary of the phenomena which presented themselves, in a tabular form. This last we consider as very desirable where minute symptomatology is concerned.

Of the 20 cases which occurred, 12 proved fatal; but among them, eight deaths took place of the first 10 who were attacked with the disease; while of the second 10, four only died. This difference, however, the authors ascribe, not to any improvement of treatment, but to the circumstance of epidemics usually becoming milder, and therefore less fatal, towards the conclusion.

The general phenomena accorded very much with those which presented themselves in London in 1832. The disease occurred in conjunction with typhous fever, acute and chronic rheumatism, erysipelas, ague, dysentery, syphilis, and gonorrhœa. Not very dissimilar combinations of the disease occurred during former epidemics in the Dreadnought; and in one of the latter cases it was remarked, that when it attacked a person labouring under *diabetes mellitus*, the urine was as completely suppressed as in the others, and the inordinate secretion had not returned a fortnight after recovery. The character of the urine is, however, not mentioned after the return of the secretion, which it should have been.

The suspension, or rather the subsidence, of severe rheumatic affections, on the symptoms of cholera making their appearance, was remarkably evinced in two of the cases narrated, in one of which, no fluid was found in a knee-joint which had been, at the time of the attack, much swollen; and in the other, very slight traces only remained, of a long-continued previous rheumatic affection, after recovery.

Bleeding, together with large and repeated doses of calomel, were the curative means principally employed; but the authors candidly admit the unsatisfactory nature of the result.

Examinations, after death, took place in 11 of the 12 fatal cases. The state of vascularity in the œsophagus, stomach, and whole of the intestinal canal was carefully noted; but neither in these organs, nor in the head, was there any thing very unusual in its character or extent. In the duodenum, the solitary glands were very conspicuous, giving the membrane more or less of a granular aspect; and conspicuous follicles were in all the cases observed in the large intestines. In the ileum, the glands of Peyer and of Brunner were much developed, particularly in the cases which proved rapidly fatal. In none of the cases which proved fatal within thirty-six hours were the contents of the small intestines found tinged with bile; but they were more or less so, in all those which were protracted. But the gall-bladder was almost invariably distended with dark bile, which some particular cause prevented passing into the intestine during the early and severer part of the disease.

The most important of the morbid appearances occurred in the chest.

" The condition of the lungs," it is stated, " varied as the patients died more or less remotely from the attack. They were found healthy, or simply congested, in four or five cases that proved fatal within thirty-six hours; while of six cases, in which the patients lived at least forty-five hours after the attack, four presented pneumonia. In one of these (8), fatal at the end of forty-five hours, the pneumonia was very partial, interlobular, and confined to the lower lobe of the right lung; in two, (4, 7), fatal at the end of 96 and 138 hours respectively, the lower lobes of *both* lungs were found in a state of red hepatisation. In all these cases, the pneumonia was latent; there were no symptoms indicating its presence, and, while the patients lived, we had no suspicion of its existence. We were not aware at the time these patients were treated, that the same observation had been made by Mr Jackson, in his Report of Cholera in Paris, in 1832. By him, pneumonia was found to exist in one-half of the cases that proved fatal after the reaction, and in all these, it was latent. This is unquestionably, as Mr Jackson remarks, the most important fact, with reference to practice, that dissection has as yet disclosed, and shows us the necessity of investigating by auscultation the condition of the lungs in all cases in which reaction has been established."—Pp. 165, 166.

The authors have examined carefully, all the circumstances which may be regarded as throwing any light on the origin of the complaint; but on this they can come to no satisfactory conclusion. The disease was insulated in the Dreadnought, and did not show itself in any of the neighbouring vessels, though the Iphigenia was moored at its stern. Its appearence, could, therefore, not be attributed solely to atmospheric influence. Neither had they any reason to imagine, that it was in consequence of infection from foreign parts; or that it was capable of exercising any contagious influence from one person to another, since none of the nurses, (all of whom lived entirely on board), or of the medical men, took the disease. The unknown influence on which it depended, seemed to act in a greater degree on the lower decks than the upper; but the hold was clean, and in no way differing from its usual state; while the ship was not more crowded than ordinary, and the scale of diet was nutritious and liberal. The shortness of the period which occurred between the appearance of the disease, in the different individuals affected by it, is considered by the authors as unfavourable to the idea of its admitting a long period of incubation. Previously to the occurrence of the cholera, typhous fever, they state, had been very prevalent; and, latterly, various acute affections of the intestines comprehending enteritis, simple fever with diarrhœa, and common diarrhœa.

Results of poisoning with Sulphuric Acid; by John Wilson, M. D., Physician to the Middlesex Hospital.—In the early part of 1834, a female swallowed some sulphuric acid, which is mentioned as being in the very indeterminate quantity of a *part of two penny worth.* After surviving the injury six months, during which time she ejected by a violent fit of coughing "a cylindrical tube about eight or nine inches in length," she left the hospital about the middle of July much recovered, but very susceptible of atmospheric influence. She had occasional expectoration to the amount of a quart of frothy mucus in twenty-four hours; much difficulty of swallowing; and occasional pain, extending from the pit of the stomach to the shoulders. In September she was readmitted, and soon began to improve; but on the 14th of November, she had a shivering fit, preceded by vomiting, and followed by continued but ineffective retchings. Dyspnœa, pain, and distress in the region of the stomach and right side; and universal and exquisite general suffering, succeeded, with swelling of the joints, and delirium. She died tranquilly on the 17th, having survived the swallowing of the acid forty-five weeks and three days.

On examination, the lower two-thirds of the œsophagus were found thickened and narrowed, with the interior vascular, irregular, and softened; "the upper third shining like an old cicatrix." In the stomach opposite to the spleen was an opening of the size of a half crown piece, with softened edges; the mucous membrane of the stomach being much softened, and the abdomen containing a quantity of dark-coloured fluid, which had escaped from the stomach; but exhibiting no appearances of peritonæal inflammation.

We have no description of the character, or opinion of the probable source of the cylindrical tube which had been coughed up. It was most likely from the trachea, as that organ was much affected; but the dissection was certainly deficient, in the circumstance of the trachea and lungs not having been examined. It is very remarkable, that peritonæal inflammation should not have come on after the effusion into the abdomen had taken place, supposing that it had occurred, according to the opinion of the authors, (which we very much doubt,) on the morning of the 14th, when the vomiting and rigors first appeared. We can hardly admit the correctness of Dr Wilson's opinion, that the state of debility which existed at the time when this incident was supposed to have occurred could account for the absence of peritonæal inflammation, on an effusion taking place into the cavity of the abdomen; besides that the patient was represented as having previously improved in looks, and increased in flesh. It would certainly have been desirable, therefore, to have ascertained with precision the state of the thoracic viscera and cavity.

Another case is given by Dr Wilson, in which a female, at about twelve at night, took two or three ounces of strong sulphuric acid, which remained in the stomach for a quarter of an hour. She was admitted into the Middlesex Hospital about ten hours afterwards, when she could only speak in a whisper; had great tenderness from the pharynx down to the epigastrium; extreme debility; frequent vomiting of a fluid of the consistence of treacle, and colour of carbonate of lime; cold extremities, and a pulse hardly to be felt. She had no convulsions; was without any evacuations; and died at twelve the same night.

On examination, fourteen hours after death, the lining membrane of the mouth, pharynx, and œsophagus was found to have a silvery gray, speckled appearance, like a snake's skin, pure particles of carbonized matter adhering to the deeper parts of the rugæ. The mucous coat of the stomach was nowhere visible, from its being covered with a black, pitch-like substance, which, when scraped off, showed the mucous coat "of a pink colour, much swollen, but entire." The commencement of the duodenum had a similar appearance, which gradually diminished, and nearly disappeared at the beginning of the jejunum.

The peritonæal coat of the small intestines, and particularly of the stomach, are stated to have been much inflamed; but it is not mentioned by what appearances such inflammation was characterized; although soon after its occurrence it hardly ever fails to be distinguished by effusion of coagulable lymph, and consequent adhesions more or less advanced.

On the use of Arsenic in some affections of the Uterus; by Henry Hunt, Esq. of Dartmouth.—The first of these cases was that of a female of 40, who was much benefited by doses of from four to ten drops, three times a day, of the mineral solution, in a cancer of the uterus, which was in a state of ulceration, and was accompanied with much fœtid discharge, and great suffering.

Recollecting the inflammation of the pudenda, which occasionally attended the employment of this agent, the author thought it not unlikely that arsenic might have some favourable operation in other diseases of those parts, and he has accordingly employed it with advantage in cases of profuse and frequently occurring menstruation; in pain or tenderness of the uterus; and in neuralgia coming on regularly at the menstrual period, and supposed to be occasioned, therefore, by some irritation in the uterus. The arsenic is given in the form of pill, in doses of about one-twentieth of a grain, three times a day; and the author found, that in cases of great susceptibility to its action, the exhibition of the pill immediately after meals has been favourable.

The last of the papers which bear more directly on the subject of practical medicine, is a *Case of Hydatid of the Liver, successfully tapped;* by William Travers Cox, M. D., of Yarmouth.—The subject of this case had, in the year 1832, twenty-one pints of fluid drawn from the abdomen, one ounce of which, when evaporated, "left half an ounce of coagulated fibrine and colouring matter, which latter was, for the most part, soluble in rectified spirit." The liver was found to be much enlarged; but under the use principally of iodine with nitrate of potash, and occasional small doses of elaterium, the patient got well, and resumed his employment of a coal-heaver.

About four years afterwards, he again came under Dr Cox's care, after having for two months had pain of the right side, with cough, dark expectoration, and fever. He was then expectorating "large dark coagula, occasionally offensive puriform matter, and large quantities of dark fluid blood." From the use of the stethoscope, the author inferred that there was "broken up structure towards the posterior part of the base of the right lung, the result of inflammation."

He went on for some months with appearances of improvement; but at length a severe return of hæmoptysis, to which he had been long occasionally subject, brought the disease to a fatal termination, in about three-fourths of a year from the commencement of the symptoms.

On examination, the upper lobes of both lungs, but especially the left, were found with small tubercles in different degrees of maturity; the base of the right lung was adherent to the diaphragm, and towards the posterior edge quite ligamentous. "On dividing this part of the lung, a small cavity was seen lined with firm, almost cartilaginous membrane, and containing clots of dark grumous blood puriform matter, and pieces of coagulated fibrine. This cavity, of almost the diameter of two fingers, was continuous with a dilated bronchial tube, whose lining membrane was thickened, and here and there ulcerated, One or two vessels opened into the cavity." —P. 335.

In the abdomen, a large cyst presented itself, which was found to be attached, "by a small portion of its surface, to the right of the gall-bladder, and the front of the transverse fissure." The cyst was about four times the size of the gall-bladder, was fully distended, elastic, and semitransparent when held to the light. On opening it, a transparent gelatinous mass was taken out, which presented exactly the shape and smooth surface of the cyst, and consisted of concentric layers, about two lines in thickness, which were very firm, transparent, and with little colour outwardly; but towards the centre were more opaque and softer; and within all had a quantity of concrete bile. Between the layers were small pieces of a red substance, resembling what is used for injecting arteries; but though this was subjected by Dr Bostock to careful analysis, that able chemist could not satisfy himself as to its precise nature.

We next come to the surgical papers, the first of which is, *On Necrosis, being an Experimental Inquiry into the agency ascribed to the absorbents in the removal of the Sequestrum. With some observations concerning the adhesion of living to dead bone.* By George Gulliver, Esq., Assistant Surgeon, Royal Horse Guards.—The author was induced to undertake the present inquiry, by the differences of opinion entertained by some of the most distinguished surgical writers, as to the process by which a sequestrum is removed. He made a great number of experiments as to the power possessed by ulcerating surfaces of dissolving bone; but he was never able to satisfy himself of the fact; and he is therefore of opinion, that, whatever may be the agency of the process of absorption, in the removal of living parts, it can no longer be regarded as the means by which the sequestrum disappears in cases of necrosis. The author reserves the consideration of the other means by which dead bone may be thrown off for a future occasion.

The second case connected with surgery is entitled *Description of a new instrument for closing vesico-vaginal, and recto-vaginal fistulæ and fissures in the soft palate.* By William Beaumont, Surgeon to the Islington Dispensary.—The description of this useful little instrument is illustrated by an engraving, without which it cannot be well understood.

Remarks on malignant diseases of the skin of the face. By Cæsar Hawkins, Surgeon to St George's Hospital.—The author limits the application of the term malignant to such diseases "as essentially possess a new structure," capable of exerting a poisonous influence either, 1*st*, upon the neighbouring textures, which are converted into a substance, either exactly similar, or at least analogous, to that of the new formation; 2*dly*, upon the *absorbent system*, so that the nearest glands become enlarged into a tumour like that originally deposited; or 3*dly*, upon the *whole constitution*, so that the poisonous secretions of the newly formed part gain access to the circulating fluids, and tubercles of various forms, but of the same or analogous character, become developed in some distant organs or textures, which have no direct communication, except through the blood, with the parts in which the new structure was formed."—Pp. 69, 70.

With this restriction, he excludes from participation in the affections which he now describes, irritable and intractable ulcers of the face; the various forms of scrofulous phagedenic ulcers; the several varieties of tubercular sebaceous diseases; and the hypertrophy of the skin of the nose described by Mr Hay, Civadier, and other writers. These various affections are considered by the authors as developements of natural textures only, by means principally of the deposits of inflammation, and as being incapable of affecting other parts of the body.

Fungous malignant diseases, whether in their hæmatoid, medullary, or melanoid varieties, present no distinctive characters, when affecting the face; but scirrhous or cancerous complaints assume many peculiarities of form, and differ, in many respects, from what is usually called cancer, in other parts of the body. The author makes some observations on the common cancer of the face, and on the cancerous ulcer, or phagedenic ulcer of the face of old persons; and then goes on to the more particular object of his paper, the consideration of what he terms the *cancerous tumour*, or *fungous cancer of the face of old persons.*

The early stage of the disease, consists of a small, round, or oval tumour, generally in the cheek, molar bone, or *ala nasi*; white in its internal texture, and lardaceous in its substance; remaining long stationary, and very seldom having pain. It often reaches the size of a walnut, without exciting alarm. It then ulcerates spontaneously, or on some slight injury; throws out healthy granulations, and pus, without bleeding or sloughing; and increases in size at its basis, but without attachment to subjacent parts. The ulceration now becomes deeper, and the bones, and subjacent parts become rapidly assimilated to the new structure, which is, in some parts, gristly, like scirrhus, but in others, softer and more pulpy. "The ulceration differs from that of *fungus hæmatodes*, as much as it does from that of common cancer, in having none of that rapid sloughing and bleeding, characteristic of tumours of that description."

"In malignancy it is intermediate between the cancerous ulcer and the common cancer;—more rapidly and extensively contaminating the surrounding parts than the former, but not having the neighbouring scirrhous tubercles, and scirrhous bands of cellular texture, met with in the latter disease, and admitting, therefore, of removal by the knife, if sufficient care be taken to excise the whole, with more chance of the cicatrix remaining sound, than in ordinary cancer,—in fact, with almost a certainty of success, where it has not attained a great magnitude."—P. 88.

In the most malignant form of cancerous disease of the face, the simultaneous developement of the poison, in other organs or textures, is comparatively rare, and excision, therefore, when there is no glandular enlargement at the time of the removal, affords every promise of a successful result.

Removal of the clavicle, with a tumour situated on that bone; by Benjamin Travers, *F. R. S.*, Surgeon Extraordinary to the Queen, and Senior Surgeon to St Thomas's Hospital.—The subject of this creditable and successful operation was a young gentleman of 10 years of age, who, after a fall out of a wheelbarrow, complained of having hurt his shoulder. Ten days afterwards, a swelling of the size of a hazel-nut, firm, but not painful, was discovered on the centre of the left collar bone. After a lapse of about two months, when Mr Travers first saw him, the tumour had attained the size of a pigeon's egg; was firm and elastic, but was not painful, except on being pressed. The increase was slow, but regular; and in about twelve months, its base occupied full three-fourths of the bone, and about two-thirds of the circumference was supra-clavicular. The skin had acquired a purple hue; pressure became more painful; and a grooved needle introduced into the upper part of the tumour sometime previously, discovered a small cavity, an inch or more in depth from the surface, from which a few drops of black grumous blood were discharged.

On consultation it was determined to remove the clavicle, and tumour connected with it, which was done on the 6th of June 1837, about twelve months from the commencement of the disease. The operation was performed with skill, and was tedious; few vessels were tied, and the loss of blood did not exceed twelve ounces. The following is the description of it.

" The little patient being recumbent, with his shoulders raised and head slightly averted, a crucial incision was made through the integument and platysma myoides, one limb of which was nearly in the line of the clavicle, and the other at right angles; and the flaps and fascial coverings successively dissected down to the external basis of the tumour. The pectoralis and deltoid muscles were then carefully detached from their clavicular origin, avoiding the cephalic vein, and the fibres of the trapezius and cleido-mastoid muscles divided on a director. One considerable vessel, in the situation of the transversalis humeri, required a prompt ligature. The circumference of the tumour was now well defined, though it was found to be firmly imbedded, and adherent on its posterior aspect, Disarticulation of the scapular extremity of the bone was next effected without difficulty, and the mobility thus communicated to the mass facilitated the completion of the operation. A director was now worked beneath the bone, as near to the sternal articulation as was practicable, and with a pair of strong bone nippers thus introduced, it was completely and clearly divided. The subclavius muscle and a part of the rhomboid ligament were now detached from the tumour, and the mass being well raised by an assistant, while the edges of the wound were kept wide apart by metallic retractors, the cervical prolongations of the tumour were separated from their remaining connections by a few touches of the scalpel, without injury to the subclavian vessels."—Pp. 137, 138.

The symptomatic fever was smart; but the convalescence proceeded without interruption, so that at the end of a month, the patient was able to be removed from London. He has since remained in perfect health; and has the same extent and variety of movement as in the other arm.

The section of the tumour, in its longest diameter, presented, for the most part, an arrangement of cells or chambers, "of pretty equal dimensions, filled with dark, solid coagula of blood, the edge of the scalpel grating, as it passed, upon particles of osseous matter. One larger compartment, deeply situated, was without a clot, having been filled with the dark fluid before mentioned. The investing membrane was evidently the condensed periosteum; the cells were the irregularly expanded cancelli; and the calcareous particles were the debris of the bony plates and walls."

With regard to the origin of the disease, the author has no doubt of its having had its rise in the cellular structure of the bone; but he does not feel himself justified in classing it with the osteo-aneurism, to which its structure affords the nearest analogy. He regards it "as a casualty attended by circumstances in which nature was unable effectually to relieve herself." Effused blood within the periosteum, acted, the author imagines, as a foreign body, and was not absorbed; while the continuous periosteum "did not secrete bone, but organizable lymph agglutinating it with surrounding membranes."

Four cases of removal of the clavicles, and four operations for osteo-sarcoma of the bone, are mentioned.

A Case of universal purulent deposition into the joints, with separation of the epiphyses, occurring as a sequel to small-pox. By Henry Ancell, Esq. Surgeon to the Western General Dispensary. The occurrence was in an infant of eleven months, and took place soon after the disappearance of the variolous eruption, which was numerous but distinct. Strabismus and other symptoms of cerebral affection, exhibited themselves towards the close of life.

Case of excision of the entire Lower-Jaw; with observations. By John G. Perry, Surgeon to the Foundling Hospital, and one of the Secretaries of the Society.—The subject of this case, when about 14 years of age, became affected with severe pain of the right side of the face, affecting all the teeth of that side, which was attributed to rheumatism, and soon extended to the other side also. At first, there was neither swelling nor redness; but in a few months, the lower part of the face increased in size, but the pain and inability to masticate diminished as the swelling advanced.

Several years afterwards, a considerable increase, both of swelling and pain took place; the parts becoming red, and acutely sensible, and having much throbbing. Matter was at length formed, and discharged itself by many apertures, to the partial relief of the patient's sufferings. She was then in her 20th year, and the case presented to the author the appearance of necrosis in the advanced stage; but on patiently waiting for the curative process of separation, it was found by examination, that the newly formed case of bone was dead, and had separated, in great measure, from the periosteum, which was in a diseased state. Under these circumstances, the author determined on removing the diseased bone, in three different portions. The first portion was that which reached a little in front of the masseter muscle on the right side, to the corresponding point on the left. This he took away by means of a saw and cutting forceps, after exposing the bone by a longitudinal incision along the basis of the jaw, in the direction mentioned. The following day, the second or remaining portion of the right side was removed without difficulty. It had somewhat descended, from the top of the support of the central part; and after an interval of three weeks, the third or corresponding portion on the left side was taken away. Little hemorrhage followed these operations; and great care was taken in each of them to avoid opening the mouth by cutting through the lining membrane; while the teeth, when practicable, were left in the gum. The wound healed without difficulty, and the patient was discharged, to follow her ordinary employment of a household servant, in a few weeks.

She is able to masticate solid food, by rubbing the morsel against the upper teeth with her tongue; for there has been no reproduction of bone in the lower jaw, to give firmness to such teeth as are left, while the new production which has taken place has not been able to resist the contraction which has risen from the healing of the wound; so that the circumference of the arch formed by the lower teeth does not correspond with that of the upper. The face is less deformed than might have been expected from so extensive a removal of a part of its solid structure.

Two other cases of necrosis in the jaw are mentioned, in one of which the coronoid process, and in the other, the alveoli of five teeth, with a part of the basis of the bone, readily separated, and were taken away through the mouth. In both instances, the cure was complete.

History of a Case of Popliteal Aneurism; with observations. By Samuel Hadwen, House Surgeon to the Lincoln Hospital.—A young man of 23, subject, by his business of a porter, to heavy exertions, was attacked with pain in his right leg, and round the knee, followed by swelling. Three months after this occurrence, when he first came under professional treatment, the right calf was found to be much larger than the other; and in the ham was discovered a distinct aneurismal tumour as large as an orange.

On July 18th 1837, Mr Hewson, whose patient he was, placed a ligature "on the superficial femoral artery, at the marginof the Sartorius muscle," which was followed by severe pain, for a few seconds, down the leg, and a slight irregularity of pulse. By the 27th, the pulsation was not perceptible in the femoral artery below the ligature, but was felt distinctly in the tumour at the ham, which was heard and resisting, but without exhibiting bruit on the application of the stethoscope. The incision had healed by the first intention, except the part through which the ligature projected. The limb was much swollen, and there were severe occasional paroxysms of pain. On the 29th, some pulsation was discovered in the middle of the thigh, and an arterial bruit extending upwards to the groin. On the 30th, there came on hemorrhage of florid blood from the nearly healed wound, to the amount of eight ounces. This occurred twice on the 31st, when it was determined to tie the femoral artery immediately below Poupart's ligament. This was done, and for two days the circumstances seemed to be improved by the operation; but on the 2d of August, considerable hemorrhage again occurred from the seat of the first ligature; and as the vessel could not be met with, for the purpose of securing it, amputation of the limb was determined upon, and immediately performed. The patient went on well till the 21st of the same month, when the wound at the groin was observed to bleed, and the bleeding recurred twice in the same day. On the 22d it took place to a large extent, and the patient then submitted to have the external iliac artery tied, which he had refused to do the day before. The operation was performed in the mode adopted by Sir Astley Cooper; and matters went on favourably till the 28th, when hemorrhage again occurred at the groin. Pressure restrained it, and a truss, in consequence, was so adjusted, as to make firm pressure on compresses of lint applied to the bleeding part. The truss was removed in two days, and very soon afterwards, the stump was nearly healed; the ligature coming away on the 29th day after its application. Convalescence went on steadily and permanently; the wound healed rapidly and securely cicatrized, and on the 101st day from the femoral artery being tied, he was fast recovering his flesh and strength.

Some observations follow, in which the author contrasts the operation of tying the common femoral artery, with that of tying the external iliac, very much to the advantage of the latter. When Mr Hodgson published his work on aneurism, he knew of 22 cases where the external iliac had been tied, without the occurrence of secondary hemorrhage in any of them; and since that time, the same operation has been very often performed with an equally favourable result.

On a successful plan of arresting the destruction of the transparent Cornea from acute purulent inflammation of the conjunctiva. By Frederick Tyrrell, Surgeon to St Thomas's Hospital, and to the London Ophthalmic Hospital.—In this communication, the author premises some observations, on the organization of the cornea, in which he considers it as distinctly established, that the vascular organization of the cornea is principally derived from the conjunctival, and little, if at all, from the sclerotic vessels. The anterior surface of the cornea is covered with a mucous, but its concave surface, with a serous coat, the vessels of which are principally derived from the iris. The nerves of the conjunctiva have their origin from the fifth cerebral nerves.

In the progress of acute purulent inflammation of the conjunctiva, "the elevation of the selerotic part of the ocular conjunctiva, by subjacent deposit of serum or fibrin, or both, renders it tense, and creates so much stress and tension on that part which is firmly bound down, over the junction of the cornea and sclerotic, that the circulation through its vessels becomes impeded and ultimately arrested, so that the principal vascular supply of the cornea is cut off, and it dies or mortifies, in part, or *in toto*." The progress to such state of things is exceedingly rapid; the author having known it to take place in eight hours, but he believes it may be in less.

From a consideration of the good effects of incisions in severe phlegmonous inflammation of the cellular tissue, Mr Tyrrell was induced to think, that a similar practice might be advantageously followed in the case of chemosis; and though he was aware that incisions had been practised in this disease, and without much success, he was induced to imagine, that the unfavourable result took place, from the incisions or excisions having been made in a direction corresponding with the margin of the cornea, and frequently extending completely over it, by which means, the vessels supplying the cornea must have been divided, and the supply of blood therefore cut off. He was hence induced to modify this plan, by making his incisions, after raising and securing the upper eyelid, as in the operation for extraction, "close to the margin of the cornea, where the tension and pressure could be greatest," and in such a way "that the direction of the wounds should correspond to the intervals between the insertions of the recti muscles; so that the principal vessels of the conjunctiva should not be injured."

The author gives several cases of the advantage derived from this plan of treatment, which he represents as having been successful beyond his expectation; and he states, that it is attended with this additional recommendation, that it prevents the necessity of the severe general depletion, which often seriously reduced the powers of the system.

It is proper to mention, that the practice has been already employed and recommended as highly beneficial by Mr Mackenzie of Glasgow.

The first of the Chemical papers is entitled, *Observations on the constitution of the urine;* by John Bostock, M. D. F. R. S.

The difficulties which have occurred to prevent that extent of knowledge relative to the pathological conditions of the urine, which might have been anticipated, have depended, the author thinks, on the peculiar nature of the urine, and its being the medium through which "all the extraneous substances are discharged from the system, which have been from any cause, formed internally, or have been introduced *ab extra;* and secondly, from the great variety of causes, both external and internal, which affect the state of the urine through the intervention of the vital actions of the system."

For the purpose of prosecuting inquiries satisfactorily in relation to the urine, the author proposes that the attention should be directed to a few well-defined objects; and the experiments made precisely in the same mode, so as to admit of ready and direct comparison with each other, and with those made on persons in the state of health. Dr Bostock has therefore given an example, in a tabular form, of the mode in which such experiments should be conducted; and we trust that his valuable suggestions will receive the attention and co-operation which they merit.

The following are the circumstances which Dr Bostock has selected for experiments, namely, "external characters, including colour, odour, clearness, specific gravity, &c.; degree of acidity referred to a fixed standard; presence and amount of albumen; amount of residuum after evaporation; proportion of residuum soluble in alcohol; amount of saline contents; amount of calcareous salts; and spontaneous changes."

The healthy urine, taking a standard of comparison, combined the following particulars. During the night ten ounces passed; it was bright, clear, citron, and urinous, having a specific gravity of 1.014; six degrees of acidity; affording 5.05 per cent. of solid contents, of which 2.6 were soluble in alcohol; not affected by heat; rendered opaque by corrosive sublimate, and when heated afforded dense flakes, amounting to 2.1 grains per ounce; three grains of precipitate by oxalate of ammonia; exhibiting the following spontaneous changes. In two days it exhibited a light cloud; and in six days became less acid, and, at the same time, somewhat opaque and turbid.

On the proportions of Animal and Earthy Matter, in the different Bones of the Human Body; by G. O. Rees, M. D., F. G. S.—The attention of the author was directed to this subject in consequence of the discrepancy which exists, in the accounts of various analysts, between the proportions of earthy and animal matter in bones. He is of opinion, that these differences may arise from every bone having a proportion of earthy and animal matter peculiar to itself; from the bones examined being differently prepared, and containing more or less of fat, which is a mere infiltration into its structure; and thirdly, from the loss of different quantities of carbonic acid during decarbonization, owing to its conversion into carbonic oxide gas, which escapes at a low heat from carbonate of lime when carbonaceous matter is present.

The experiments brought forward were conducted on bones from the same adult individual, similarly prepared; freed from fat, periosteum, and cartilage, and decarbonized: but in order to supply the loss of carbonic acid gas, which decarbonization produced, the result was moistened with a solution of sesquicarbonate of ammonia, and the heat subsequently applied to low redness.

The results of the foregoing analysis are as follows:

" *1st.* The long bones of the extremities contain more earthy matter than those of the trunk.

" *2d.* The bones of the upper extremity contain somewhat more earthy matter than the corresponding bones of the lower extremity; thus the humerus more than the femur, and the radius and ulna more than the tibia and fibula: this difference is, however, small, being about one-half per cent.

" *3d.* The humerus contains more earthy matter than the radius and ulna; and the femur more than the tibia and fibula.

" *4th.* The tibia and fibula contain, as nearly as possible, the same proportions of animal and earthy matter, and the radius and ulna may also be considered alike in constitution.

" *5th.* The vertebra, rib, and clavicle are nearly identical as regards the proportion of earthy matter; the ilium containing somewhat more of earths, the scapula and sternum somewhat less; the sternum containing more earthy matter than the scapula.

" *6th.* The bones of the head contain considerably more earthy matter than the bones of the trunk, as observed by Dr J. Davy; but the humerus and other long bones are very nearly as rich in earths.

" *7th.* The metatarsal bones may probably be ranked with those of the trunk in proportional constitution."—Pp. 409, 410.

Cancellated structure the author found (at least in the ribs) to contain less earthy matter than the more solid parts of the bone.

Several of the conclusions relative to fœtal bones correspond with those now stated; but the bones in the upper extremity contain rather more bony matter than those of the lower; the humerus contains more earthy matter than the radius and ulna; and the femur than the tibia or fibula; while the ileum possesses somewhat more, and the scapula somewhat less earthy matter than the clavicle or ribs.

Comparing the relative proportions of earthy matter in fœtal bones with those of adults, there is not much difference; except that in the former there is not that excess of earthy matter in the long bones and in the bones of the head, which is observed in those of the latter.

The only paper of a physiological character is, *the History of a female who has four mammæ and nipples.* By Robert Lee, M. D., F. R. S., Physician to the British Lying-in-Hospital, and Lecturer on Midwifery at St George's Hospital.—This singular malformation had been concealed by the subject of it till her first confinement; and she was very reluctant to expose it to the author, ten years afterwards, when the state of her health consequent to a premature delivery rendered this necessary. "After long entreaty," says the author, "I obtained leave to inspect the breasts, and was surprised to find that there were two on each side, as had been represented; the two of the same side being separated by a deep oblique depression. The inferior or pectoral mammæ, as they were afterwards termed by Sir Astley Cooper, were fully developed in the natural situation, and their nipples, areolæ and glands presented nothing unusual in their appearance. Near the anterior margin of the axilla, a little higher up on each side, was situated another mamma, about one-sixth the size of the others. The nipples of these were small and flat, but when gently pressed, a milky fluid, which had all the external characters of the milk secreted by the other breasts, flowed copiously and readily from several ducts which opened on their extremities. When milk was drawn from the lower breasts, a small quantity usually escaped from the nipples of the superior breasts, and when the draught came into the former, the latter invariably became hard and distended." —Pp. 266, 267.

The lady had borne several living children previously, but, from the flatness of the nipples, was unable to suckle them. The mammæ were considered by Sir Astley Cooper (who viewed the case as without a parallel in this country) as completely separate from each other. When she afterwards became pregnant, and was delivered of a living child, she was able to suckle it with the pectoral breasts.

Five similar cases are quoted by the author from foreign journals; and he states that in some women only one breast has been developed; others have had two nipples on one mamma, and a few have had three breasts, two in the natural situation, and a third situated between the other two. Only one case, he adds, has been recorded of the occurrence of five mammæ in the human subject.

On subjects in Morbid Anatomy and Pathology there are five papers.

Facts and inferences relative to the condition of the vital organs and viscera in general, as to their nutrition in certain chronic diseases. By John Clendinning, M. D., Physician to the St Marylebone Infirmary, and one of the Secretaries of the Society.—In considering the bearings of the faculty of nutrition, which the author regards as the leading function of the animal body, he is disposed to refer the most serious diseases to such as involve some lesion in that function, while the remainder, (supposing that there were two groups in the whole,) would consist but of few leading acute maladies, and almost no dangerous chronic ones.

The author's object, in the present paper, is to offer some contributions to the history of diseases of the heart and lungs, many of the most important of which are greatly dependent either on excess or defect of nutrient action. Over-nutrition, or hypertrophy of the heart, he regards as one of the most common of fatal diseases; and inflammation and tuberculation of the lungs, as being the result, the former of active or excited, the latter of vitiated nutrition. Diseases of the heart are viewed by the author as more frequent in their occurrence, and more fatal in their issue, than pulmonary consumption itself; and as the "main cause of the great bulk, if not the whole of the sufferings and mortality ascribed by authors to numerous chronic diseases," comprising a great variety of affections of the chest and liver. He is also disposed to attribute to a similar origin, "no small part of the gravity and fatality of acute diseases of all the great viscera in adult subjects, but more especially of those of the lungs, as well as of typhous and other continued fevers." Dr Clendinning hardly expects such an extent of deduction to be readily admitted; but he hopes before long to be provided with sufficient data in proof of his positions. The facts brought forward in the present paper, principally consist of measurements by weight, of nearly all the principal viscera in most cases, and of the persons in many, of 249 subjects, of whose diseases and dissections, the author is in possession of satisfactory memoranda. He arranges the whole in tables, which he accompanies with appropriate explanations, and many general and interesting remarks.

The bearings of his deductions in respect to pulmonary consumption are first considered. In this disease, though there is a general wasting of the body; yet it is not general, but partial. It falls upon "the organs of locomotion, and external parts principally, if not exclusively;" and the viscera, which, from their importance, might be supposed to be among the parts most seriously affected, by no means suffer in an equal proportion. Even the lungs themselves are considered by the author as receiving and converting into their own substance, rather more than less the regular proportion of blood; and he is of opinion, that they do not suffer from "want of nutriment supplied, nor from any sluggishness in the business of assimilation, but from the preponderance of disintegrating processes peculiar to their structure." In most of the organs, the average weight was found to be as high, or even higher, in phthisical patients than healthy ones; and yet in two of his tables, where females only were concerned, though the average weight of the body was 82 lb. in those who did not die of consumption, it was only 66 lb. in the phthisical.

In the bodies of persons who have died of diseases of the heart, the same circumstances apply still more strongly; for here the average weight of the brain was found in males, as appears by the table, to be one twenty-fifth heavier than the standard; of the heart two-fifths heavier; the liver one-ninth; the kidneys one-fourth; while the spleen, pancreas, and stomach, were each of them also rather heavier, though in a small degree only, than the average of the same organs in persons who died of other diseases than phthisis, and those of the heart. Similar inferences are deducible from the tables of females.

As life advances, all the organs of the body lose substance, with the single exception of the heart, which, after the age of sixty, instead of diminishing with the person and the viscera, increases, so as to exceed the average by about a twelfth part. In this conclusion the author states his having been supported by the researches of Dr Bigot of Geneva.

On Aneurisms of the Heart, with Cases; by John Thurnam.—Partial dilatation, or aneurism of the heart, though long known, has been more particularly attended to since the publication, by M. Breschet, of ten cases of the disease, in 1827. Similar diversities of opinion have been entertained in relation to aneurism of the heart, which have taken place on the subject of those of arteries; but the author is of opinion, that while in some instances the disease may have the character of a *false* aneurism, produced by rupture or ulceration of one or more of its coats, in others it has the nature of a *true* one, and arises from dilatation alone. The seat of aneurisms of the heart is known by much the most frequently in its left ventricle, the right being, as far as is at present known, altogether exempt from it. But aneurisms may likewise occur in the left auricle; and as the valves themselves have been occasionally found to be the seat of dilatations, the author regards those in such cases as likewise affording examples of aneurism.

He gives the details of several interesting histories of these varieties of aneurism; and refers, in an appendix, to many of the principal examples of the disease.

From the materials thus collected, derived from 84 cases in the whole, and a diligent inspection of every preparation to which he could obtain access, the author has been able to communicate an interesting account of the history of the disease. The absence of aneurisms in the right ventricle of the heart has been attributed, by Breschet and Cruveilhier, to its apex being much stronger than that of the left ventricle, in respect to the thickness of its lateral parietes, and, therefore, better able to support an equal degree of distending force. But Mr Thurnam attributes this exemption, partly to differences in the functions of the tricuspid and mitral valves, the former admitting regurgitation, and therefore operating, as has been said, as a sort of safety-valve in the circulation, and partly to the right ventricle, as the circulator of black blood, being the centre of the nervous system, as the left is of the arterial, and possessing, therefore, much more of a venous than an arterial character. Of the 84 cases, which form the materials of the author's deductions, 58 were in the left ventricle; and these afford 35 examples of tumour exhibiting itself in the exterior of the organ. Aneurismal sacs vary much in magnitude, from the size of nuts to that of the whole heart itself; and they usually, when of some standing and magnitude, open into the ventricle by a narrow mouth. In 15 of the cases, the sacs were formed by muscular fibres and pericardium; in four, by endocardium and pericardium only; in 25, by the whole of the structures combined; while in the remainder, the component parts were incapable of being made out. In a small number of cases the tumour of the heart, by its attrition against the opposed surface of pericardium, produced limited inflammation, and consequent adhesions; while in about six, where there was not this extra support, a fatal extravasation of blood took place into the cavity of the pericardium.

With regard to the contents of the sacs, 23 of the cases had laminated coagula, 17 simple amorphous coagula, and 19 were empty. The most frequent position of the aneurism was near the apex. In 52 of the 58 cases, one aneurism only existed; in six cases there were two in each; in one, three; and in another, four incipient aneurisms.

Respecting the pathological changes which take place in the heart, when the seat of aneurism, they are mostly to be attributed to the effects of inflammation; and hence the author is of opinion, that it may fairly be inferred, that aneurism of the heart is not dependent on changes in one of its tissues only. Dilatation, or dilatation with hypertrophy, occurred in a small number of the cases. The proportion of cases presenting themselves in females is much greater than in arterial aneurism; and with regard to age, the most frequent occurrence of the disease is between 20 and 30, and again in advanced life. The author is disposed to consider "by far the greater proportion of cases" as having the nature of "*true* aneurism," when there has been a dilatation of the parietes of the heart, in consequence of organic changes in the tissues composing it, and which appear to have been the result "of more or less active antecedent inflammation." On one or two occasions, the author observed what he regarded as the commencement of the changes which end in true aneurism, namely, enlargement of the natural interspaces, or depressions between the smaller fleshy columns. He is of opinion, however, that the occasional occurrence of false aneurism, by partial ruptures of the heart, is not to be doubted; but there never has been a case in which this form of disease has originated "from ulceration, and the discharge of the contents of abscesses and cysts into the cavity of the ventricle."

In the great majority of cases, the disease is of very insidious origin; the duration is exceedingly various, and the mode of death much diversified. Dyspnœa, venous congestion, passive hemorrhagies, and dropsy, arising from imperfect contraction of the heart, are usual symptoms, together with a variety of distressing sensations in the præcordia. In some instances, the pain was accompanied by a peculiar and distressing sense of weight. The diagnosis is difficult, but most likely to be successfully effected, the author thinks, by means of acoustic and manual examination. A letter from C. J. B. Williams on this subject will be read with interest.

With regard to aneurism of the auricles, it bore the character of true aneurism, as the cases, nine in number, were connected with, and dependent upon, an extreme contraction of the mitral orifice, producing a difficult transmission of blood from the left auricle. In three cases, the *right* auricle was the seat of a lesion analogous in many respects to aneurism of the left auricle.

Aneurism of valves of the heart, which is the last division of the subject, usually arises from dilatation, and have, therefore, the character of true aneurism; but when they are connected with destruction of one of the laminæ of the endocardium, they may be regarded as false ones. Under any circumstances, they are unfavourable to the formation of coagula. The diagnosis, the author is of opinion, will, for practical purposes, resolve itself into that of obstructive and regurgitant valvular disease, which has been well treated by Drs Hope and Williams.

On increased thickness of the Parietes of one of the ventricles of the Heart, with diminution of its cavity; by George Budd, M. B. F. R. S. Fellow of Caius's College, Cambridge, and

Physician to the Seamen's Hospital, Dreadnought.—M. Bertin, in 1811, designated as "Concentric Hypertrophy," unnatural thickness of the parietes of one of the ventricles of the heart; but M. Cruveilhier is of opinion, that this view of the subject is an erroneous one; and that the obliteration of the cavity of the ventricle and the proportionally increased thickness of the parietes, are only the result of the mode of death. All the hearts which he had an opportunity of examining, of persons who died by the hands of the executioner, presented this double phenomenon in the highest degree; and hence he infers, that the hearts described as concentrically hypertrophied were merely hearts more or less hypertrophied, which death surprised in all their energy of contraction.

Dr Budd considered it to be important to examine with attention these contending opinions; and from the result of many careful inspections, of which he gives the details, he is led to adopt M. Cruveilhier's view of the subject, and to conclude, that there was not really a contraction of the cavity during life, for the ventricle became relaxed to its ordinary capacity, after the heart had been a few days in maceration; and relaxation likewise took place by the forcible introduction of the fingers. There was likewise "no intermittence or irregularity of the pulse, no dilatation of the right cavities or dropsy; symptoms of obstacle to the circulation through the heart, which must have occurred had the cavity during life been so small as it appeared to be."—P. 316.

He likewise infers from the same cases, "that enormous hypertrophy, unaccompanied by dilatation, or by disease of the valves, does not produce any of the symptoms characteristic of an obstacle to the circulation through the heart.

With regard to the cases described by him, in which there was extensive valvular disease, the author is of opinion, that "the diminution of the cavity cannot be explained by the hypothesis of an obstacle behind it; and, in some of these cases, the existence of an obstacle before it renders it highly probable that this diminution was merely a passing condition of the ventricle: and as the appearances of concentric hypertrophy were not more marked in these cases than in those of the former category, and as the symptoms of obstacle to the circulation, observed in these cases, were such as would result from the diseased valves alone, we cannot admit the existence of concentric hypertrophy in the category we are now considering."—P. 317.

"Concentric hypertrophy of a ventricle," he adds, "in a high degree, with obstruction at its discharging orifice, and an extraordinary passage for the blood, occasionally exists as a congenital malformation, and, in most cases, the right is the ventricle so affected;" and that "hypertrophy of the heart, to whatever extent it exists, when it is exempt from dilatation of the cavities, and from disease of the valves, does not produce any of the symptoms of an obstacle to the circulation through the heart."—P. 317.

On black expectoration and the deposition of black matter in the lungs, particularly as occurring in coal-miners and moulders in iron works; by William Thomson, M. D. Fellow of the Royal College of Physicians and Surgeons of Edinburgh. Part 2.—The first part of this paper was published in the 20th volume of the Society's Transactions, of which an account was given in our 49th volume, p. 256. Its object is to bring to notice a "number of instances of black discoloration of the sputa, observed during life, and of black infiltration of the lungs and bronchial glands, ascertained after death, in persons who, from their occupations, were particularly exposed to the inhalation of carbonaceous powders or gases, such as coal-miners and moulders in iron-works."

In the present communication Dr Thomson recapitulates the observations and opinions entertained by authors respecting black sputa and black deposition in the pulmonary organs. Concerning the sources of such black matter opinions have varied much; some having regarded it as a secretion from the bronchial glands, or from those of the mucous membrane of the trachea, while others have viewed it as originating either from the exhalents of the pulmonary air-cells, or as being entirely of extraneous origin. The statements brought forward have been collected with judgment and industry, and will be perused with interest.

From these statements, as well as from information which he has collected, and which he gives us in his paper, it appears, that black deposition is capable of occurring in individuals whose occupations did not render them peculiarly liable to accumulation of carbonaceous matter in the respiratory organs, and in whom, at the same time, there appeared to be no melanotic diathesis. The author has likewise found, from inquiries which he had made among many individuals of large experience among colliers, that they had not observed in that class of persons appearances such as were described by him in his former paper.

In a subsequent communication the author means to follow up the subject, by bringing forward some general conclusions, deducible from the evidence which he has brought forward in this and his previous paper.

Account of a case of enormous Ventral Aneurism; with the post mortem appearances; by Sir David J. H. Dickson, M. D. F. R. S. Ed., F. L. S. &c. Physician to the Royal Naval Hospital, Plymouth.—This was the case of a person of 36, who had many symptoms of a paraplegic character, together with an ill-defined, pulsating tumour in the left side of the abdomen. This was considered to be either a tumour in contact with the abdominal aorta, or an aneurism of that great trunk, or of the common iliac artery; and it was supposed that its pressure on the vessels and nerves, and especially on the hypogastric plexus, occasioned the various paraplegic symptoms which exhibited themselves.

The tumour increased in size, though the general health improved; but on a little exertion of ascending stairs, "he was seized with excruciating pain in the right iliac region, followed by excessive faintness, and a death-like paleness of the countenance, indicating the rupture of the aneurism, and, after suffering much pain, he expired in about three hours."—P. 402.

On examination, an effusion of blood in vast quantity, was found to have taken place under the posterior reflexion of the peritoneum. It had escaped "by an ulcerated opening of the size of a shilling, in the side of an immense tumour near to the right kidney, which it had displaced forward and laterally; and which, on further examination, proved to be an enormous aneurism of the descending aorta. The aneurismal dilatation, upon further investigation, was found to commence from the posterior part of the artery, two inches above the cœliac axis, by a kind of neck, which extended to two inches and a half above its division into the iliac trunks; where, suddenly bulging out, it expanded over the whole of the abdomen. The tumour was so immense indeed, that with the exception of the cæcal region, from which it diverged to the left, it might be said to occupy the epigastric, both hypochondriac, the umbilical, and left iliac regions, and the pelvis."—P. 403.

The tumour was filled with coagulated blood, and its lining was of a vivid red colour, mottled with osseous scales, which prevented its collapse.

ART. IV.—1. *On the Statistics of English Lunatic Asylums, and the Reform of their public Management.* By WILLIAM FARR. 8vo. London, 1838. Pp. 46.

2. *Observations on the Management of Mad-Houses, illustrated by occurrences in the West Riding and Middlesex Asylums.* By CALEB CROWTHER, M.D. Formerly Senior Physician to the West Riding Pauper Lunatic Asylum. 12mo. London, 1838. Pp. 145.

OF the two works now before us the first evinces great industry and judgment in collecting and digesting a number of facts not very easily obtained regarding the English asylums. The other, though written rather in a censorious vein, contains a number of statements which deserve at least some attention, and perhaps investigation as to their accuracy. That our readers, however, may have an opportunity of judging for themselves, we shall present them with a brief abstract of the contents of each, reserving our remarks on the subjects discussed in them for a future paper, which, in a previous article, we have already promised.

The greater part of Mr Farr's work appeared some time ago in the pages of the Annals of Medicine, a weekly periodical, now discontinued.

In some introductory remarks, the author complains of the imperfect manner in which the management of the insane is still conducted in this country, and of the difficulty of procuring accurate returns of the state of lunatic asylums and their inmates.

> " Accurate returns are not published, the commissioners into whose hands they are committed carry on their proceedings in secret, and are not, in the proper sense of the word, responsible. Having had occasion to ascertain the names of the metropolitan commissioners of lunacy, for publication in the British Medical Almanack, I sent two or three times to the office in John Street, Adelphi, for a list, and at length applied personally. Mr Du Bois's clerk stated that *the commissioners did not wish to be known, as they were very much pestered by personal applications from the friends of lunatics, which was annoying*, as they rendered their services gratuitously. They wished all applications to pass through Mr Du Bois's hands. They were nearly all noblemen and gentlemen, not paid for their services. The *greatest secrecy was necessary in their proceedings*, as many of the lunatics that came under their cognizance were related to the aristocracy, deeply concerned in concealing the fact that insanity existed in their families."—Pp. 1, 2.

Mr Farr then points out the evils attending this arrangement. An unpaid functionary, especially when acting in secret, may be looked on as almost, if not altogether, irresponsible, and though, from the personal character of the present commissioners, nothing reprehensible is for a moment to be suspected, yet, keeping before us the evils formerly perpetrated, and the possibility of worthless or careless individuals being hereafter appointed, we agree with the author in thinking that a greater degree of publicity should be given to their proceedings than has yet been done, and that a statistical report, embracing all particulars which can possibly be ascertained, should be published annually.

Mr Farr next proceeds to the discussion of the subject of his paper,—Pauper Lunatics and County Lunatic Asylums. On the 14th July 1836, the House of Commons ordered a paper to be printed, entitled " Returns to an Address of the Honourable the House of Commons, dated 28th March 1836, for a return of the total number of patients admitted since each county lunatic asylum was opened, distinguishing the number admitted each year, as well as the number of readmissions during the last five years; the number of permanent cures each year; the number of deaths each year, and the assigned cause of each death; the dietary; the mode of occupation, with the number of patients employed in each; the number and distinction of medical officers; the number of male and female keepers, and if there be a separate infirmary for males and females, the weekly charge for each patient; and if other than pauper patients are kept in the asylum; to be made up to the end of last year for the five preceding years, or since the opening of the asylum."

This return might have furnished much important information, notwithstanding its deficiencies, had no question been put as to the number of permanent cures. If the number of those discharged cured, and the number of those readmitted had been demanded, intelligible answers would have been obtained, though even then the amount of real cures could not have been ascertained, since there would have been no means of knowing how many of those discharged as cured were afterwards sent to other asylums, and how many were in a short time removed by death. As it is, it is difficult to understand the returns. After a careful examination of them, however, Mr Farr has constructed a table in which he assumes that the numbers "admitted" include the readmitted; that the number of permanent cures indicates the total number of recoveries, minus the numbers readmitted into the particular asylums by which the returns were respectively made; and consequently, that the number of permanent cures added to the numbers readmitted is equal to the total recoveries.

Fourteen of the English counties have lunatic asylums, twenty-six have none, the insane being kept either in private establishments, workhouses, or allowed to be abroad. There is but one county asylum in Wales, which in fifteen years has received two patients and cured none.

"The annual admissions into the fourteen county asylums have been for the last five years 1138; the total admissions 5477; the cures 2199; the deaths 1562. Calculated on the admissions, the cures therefore were 40.2, the so-called permanent cures 28; the deaths 28.5 per cent. As the average number of lunatics in the asylums was not demanded, neither their population nor any of the interesting relations this bears to the admissions, cures, and deaths, can be determined."—P. 4.

The following are Mr Farr's conclusions respecting the number of pauper lunatics.

"From the last report of the Poor Law Commissioners, it appears that in 106 unions, with a population in 1831 of 1,336,471, the total number of paupers relieved, January—March 1836 was 106776; in the same period the number of idiots was 1052; the number of lunatics 565; of the latter 126 were relieved in doors, 196 out doors, 243 were in lunatic asylums. I do not exactly understand the statistical tables published by the Poor Law Commissioners, but assuming here that the numbers 1052 and 565, represent the number of pauper idiots, living in the unions at one time, it will follow that in a population of 100000, there are 78 pauper idiots, and 42 pauper lunatics, equal to 120 of both classes. The idiots are to the lunatics as two to one. If these proportions hold, and the population of the kingdom be 14 millions, it will follow that among the paupers there are 10920 idiots, 5880 lunatics, equal to 16800. As 43 out of 100 pauper lunatics are in asylums, 2920 in all are lodged in such establishments. In the Stafford asylum, 634 lunatics were admitted in the five years 1831–5; while the mean number resident was 187; the mean term of confinement was therefore a year and a half, (1.473 year). Taking all the asylums except Hanwell, it is probable that the time of residence at Stafford is below the average; the patients remain as long as at Gloucester. Now if we add to 564 at Hanwell in 1834, the 901 admitted annually at the other county asylums * × 1.72, 2113 will be obtained as the average population of all the county lunatic asylums. Of 2920 pauper lunatics, 2113 are confined in the county asylums,† 787 in other establishments. These calculations can be easily appreciated; they are not the precise truth; but must be considered the nearest approximation that can be obtained with the present data; and serve till Parliament furnishes us with better returns. There are innumerable omissions in the returns of the number of insane poor made to Parliament; all agree in this; yet of 12547 insane persons 'ascertained beyond all doubt, as existing in England,' by Sir A. Halliday, (5741 idiots, 6806 lunatics,) 11000 were paupers. No one will be astonished that half the idiots are omitted in this estimate, when Sir A. Halliday's results make the idiots not so numerous as the lunatics in England; while in Wales the number of the former is to that of the latter, as 7 to 1."*—P. 5.

Respecting the proportion of cures in lunatic asylums, Mr Farr states, that, out of 1183 patients treated at Hanwell, only 222 were cured; while of 4294 patients treated during the same period at the other county asylums, 1977 were cured, the per centage of cures at Hanwell being thus only 18.8, while at the other asylums it was 46.0. Sir William Ellis explains this by asserting, that nearly all the patients admitted into the establishment under his care were old incurable cases. It would have been more satisfactory had he shown in what respect the cases admitted at Hanwell differed from those received into other county asylums. The following table, compiled from various sources by Mr Farr, exhibits the total admissions and reported cures in twenty-six asylums, as well as the proportion of recoveries out of 100 patients during their stay in the respective institutions.

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* All the patients in the county asylums are not paupers, but the paupers probably remain rather longer than 1.72 year, and would keep up a population of 2113, although less than 1138 were annually admitted."

† "The estimates of the number of insane persons in England, made by Dr Burrows and Sir A. Halliday, are much below the mark."

Asylums.	Date.	Cases admit. or treated.	Cured.	Recov. p. 100.
County Lunatic Asylum of England,	1831–35	4,294	1,977	46.0
Hanwell Asylum, . .	1831–35	1,183	222	18.8
Total county Asylums England,		5,477	2,199	40.2
Salpêtriere and Bicêtre, .	1801–21	12,592	4,968	39.5
St Luke's Hospital, .	1751–1834	15,402	6,117	39.7
Bethlem, . . .	1823–24	2,661	1,230	46.2
Radcliffe Asylum, Oxford, .	1826–35	304	117	38.5
Nottingham General Lunatic Asylum,	1812–34	1,058	449	42.4
Manchester Lunatic Asylum,	1766–1834	2,789	1,066	38.3
York Lunatic Asylum, .	1814–36	1,074	367	34.1
Lincoln Lunatic Asylum, .	1820–35	457	175	38.3
Five English Asylums, .		5,682	2,174	38.3
Charenton, Paris, .	1826–33	1,557	518	33.3
* Senavra, Milan, .	1802–26	6,006	3,516	58.5
Retreat, near York, .	1812–33	334	168	50.3
Total, . .		49,711	20,890	42.2
Total exclusive of Senavra,		43,705	17,374	39.7
Tot. Excl. of Senavra and Hanwell,		42,522	17,152	40.5

The proportion of recoveries in asylums under the common treatment now pursued may therefore be stated to be 40 per cent. of the cases. Setting aside, however, the Retreat and Hanwell, the highest proportion was 46.2, the lowest 33.3, and the mean of these two 39.75, within a fraction of 40.34. The extent of oscillation on both sides of the medium, 40.30, was equal.

Between the rate of recovery in the English, and that in the Irish asylums, there is a remarkable agreement, as the following table will show.

	Date.	Cases.	Cures.	Recoveries per 100.
District Asylums, Ireland,	1834 & 37	1589	722	45.4
Royal Naval Hospital Haslar,	1829—34	172	83	48.2
Bethlem,	1784—94	1664	574	40.5

"The influence of age on recovery is shown by the following results, deduced from observations made at Bethlem 1784—94, by Dr Haslam, (Observations on Insanity.")

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* The majority of the patients are of the poorest class, who, from the influence of marsh *miasma* and the worst sorts of food, become affected with *pellagra*—a species of cachexia inducing mental derangement. Purer air, wholesome lodging, and a good diet, recover, with very little or no medical aid, a large proportion of these poor people.

Ages.	Admit.	Discharged Cured.	Incur.	Cured per 100 Fact.	Theory.
10—20	113	78	35	69.	
20—30	488	200	288	41.	42.
30—40	527	180	347	34.	32.
40—50	362	87	275	24.	24.
50—60	143	25	118	17.	18.
60—79	31	4	27	13.	14.
Total	1664	574	1090	40.	

" When grouped in three periods, the rate of recovery follows a very regular progression; 68 per cent. It decreases 33 per cent. every 10 years (20—70.)

Ages.	Fact.	Theory.
20—40	37	37
40—60	22	22
60—	13	13."

Respecting the probability of recovery in different stages of madness, Mr Farr states, that the general law in acute diseases is, that the danger diminishes every day from the time of the attack; while in insanity the probability of ultimate recovery diminishes in the same ratio. The observations published on insanity not being arranged in proper forms, they are in the present inquiry little better than useless, or useful only by accident. A series of observations, however, by M. Esquirol, shows year by year the number of recoveries and deaths in 2804 female lunatics admitted during ten years into Salpêtrière, and from calculations founded on these Mr Farr concludes that, of the cases taken at the time of entering the hospital, 43 per cent. recovered; of those remaining at the end of the first year, 34 per cent.; of those remaining at the end of the second year, 11 per cent., and that thus the probability of recovery goes on decreasing yearly.

" When Sir W. Ellis, therefore, proves that lunatics enter Hanwell, on an average *two years* later than they enter the other county lunatic asylums, or any other known asylum, he will exonerate that institution from the serious charge of curing but 19 cases out of 100 treated. At the Retreat near York, of 100 cases admitted 0—3 months after the attack, 81 recovered; 3—12 months 43; and afterwards 31."—P. 8.

In estimating the duration of attacks of insanity, it must be borne in mind that patients have in general been ill for some time before admission, and that several leave before recovery, so that the mean term of residence is not equal to the mean duration of the cases. Some of the shortest cases again are not sent to asylums.

" *The mean duration of treatment in asylums may be deduced from the number annually treated, and the mean number constantly under treatment.* If a hundred cases are admitted annually into an hospital, and the mean number constantly resident is twice that number (200) each patient must remain on an average two years."

The following table shows the duration of treatment and proportion of deaths in each of six asylums:

Asylum.	Period.	Admit. or treat. ann.	Mean No. resident.	Duration of treat. in years.	Annual deaths.	Deaths out of 100 constantly living.	Deaths out of 100 admitted.
Staffordshire,	1831–5	127	187	1.47	22	11.8	17.3
Nottingham,	1834	60	94	1.57			
York, W. Riding,	1836	50	158	3.16	7.	4.4	14.0
Lincoln,	1820–35	28.5	49	1.72	5.56	11.4	19.5
Gloucester,	1824–32	49.5	85.1	1.72	6.1	7.2	12.2
Hanwell,	1835	141	580	4.11	71.0	12.2	50.4

" Thus, at the Stafford and Lincoln Asylums, where the time of observation was considerable, the mean duration of treatment was within a fraction of one year and a-half; in this time 46.5 per cent. recovered at the Stafford Asylum; 38.3 per cent. at Lincoln; while at Hanwell the patients were kept more than four years, and in that time only 19 per cent. recovered. The term of treatment is of very serious importance in an economical view. The *treatment* of a patient at Hanwell would cost the parishes (6s. 7d. a-week for 4.11 years) L. 70; at Stafford Asylum, L. 19; the *cure* of a patient at Stafford would cost L. 41; of a pauper at Hanwell, L. 373; because after 100 had been treated four years, only 19 of the number would be cured at Hanwell: As much depended, perhaps, on a difference in the patients as on any difference in the treatment."—P. 9.

From the extensive data of a table prepared by Mr Farr, to exhibit the duration of treatment and the mortality at Bethlem, St Luke's, and Senavra, it appears that the mean duration of treatment at Bethlem and St Luke's is very nearly *one year.*

" There are unwise and unusual regulations at both hospitals, making the discharge of patients that have been under treatment twelve months compulsory; hence the class absurdly called *curables* only remain from .53 to .72 of a-year (193—263 days) when some 30 or 40 per cent. of them are discharged *uncured.* The males do not remain under treatment so long as the females at either hospital; the difference at Bethlem is 31, at St Luke's 32 days. The 'incurables' 10½—13⅓ years; but at both hospitals males remain longer under treatment than females—the contrary happening among the 'curables.' At the Senavra Hospital, the term of treatment (1.68 years) agrees with that in several English hospitals. Male criminals are confined twice as long as females at Bethlem."

From calculations founded on the county returns, Mr Farr comes to the conclusion, that it is *two* to *one* that a patient recovered from insanity will not have a relapse.

It has hitherto been a disputed point, whether the mortality is greater among the insane than among the population in general. From the facts, however, collected by the author, it would appear that insanity increases the mortality sixfold. It is possible that the mortality of patients accumulated in asylums may be greater than that of the entire class of lunatics; but after making every deduction, the mortality of the insane will be incomparably higher than that of the general population. The mortality increases with age.

Passing over Mr Farr's observations on the fatal diseases and treatment of lunatics, we find the following remarks on the diet of the insane in county hospitals:—"The number of meals is generally three, breakfast, dinner, and supper. Luncheon is given at Lancaster; and to patients that work at Kent and Nottingham. At Lancaster, Nottingham, and Stafford, the dietary for males and females is different. Meat is allowed at all the asylums. It is generally served at dinner, three or four days a-week. The diet of the sick is regulated by the medical officers. No very accurate comparison can be instituted between the dietaries, but a simple glance at the table* will show that they vary to an extent, which cannot fail to affect the state of the patients for good or for evil. At the Gloucester, Kent, Nottingham, and Stafford asylums, the patients have nearly three pounds (2.87) of solid cooked meat and cheese every week; at York, Suffolk, and Hanwell, little more than a pound. The bread varied from five pounds to ten; pudding, flour, &c. to the amount of one to four pounds, should be added to this item. Potatoes are the principal vegetable; but the amount of vegetables is seldom stated. At Norfolk the average weekly allowance is mentioned in the return; it was seven pounds. At Lancaster the diet is quite peculiar. The patients have *scouse* five days a-week for dinner; pottage of bread, oatmeal, flour, milk, for breakfast and supper. The weekly quota of potatoes is twelve pounds. The adequacy of the dietary depends, to a considerable extent, on the amount of nutritive matter in the slops. The dietary at Hanwell stands low in the scale; six ounces of meat are given three days a-week, soup made from the meat boiled the day before, three days; stew from the coarse meat one day, for dinner; for breakfast and supper, at each a pint of oatmeal porridge, containing one-third of a pint of milk, one ounce and a half of oatmeal, and half an ounce of flour. It would be better to vary this, night and morning. The weekly quantity of solid food, exclusive of vegetables, was ten to twelve pounds at the asylums; and about 20 per cent was animal matter. It was only 8.65 pounds at Lancaster, but there was an extra amount of potatoes by way of compensation. The food at Stafford and Nottingham was in every respect above the average. The liquids generally amounted to twenty-one pints in the week; or three pints on an average daily. The total amount of water in soup, &c. is not given at Lancaster and some other places; fourteen pints of beer are drank weekly by the patients at Gloucester, Kent, Nottingham, and Stafford; no beer is allowed at Hanwell or York. The dietary of the keepers is only mentioned at Lancaster and Kent. Strength is required in these people; and a very firm conviction seems to have been entertained that it depended in a great degree on the amount of food—particularly animal food.

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* We have omitted the table.

	Meat, &c.	Bread, &c.	Potatoes.	Solid food.	Relations.
Keepers at Lancaster Asym.	6·5 lb.	13·5	14·0	34·0 lb.	1·00
Patients, .	1·5 lb.	7.1	12·0	20·6 lb.	0·61

If the keepers are on full diet, the patients are on half diet; if full diet be 1, the patients are on diet = ·61; or calculated on animal food alone on diet = 0·23. This mode of expression is new; but some notation of the kind is necessary, if there be such a thing as Count Rumford called a 'Science of Nutrition.'

	Meat, &c.	Bread, &c.	Bread and Meat.	Relations.
M. keepers at the Kent A.	8·50 lb.	10.50	19·0 lb.	1·00
Patients, .	2·87 lb.	7·69	10.56 lb.	0.55

Here the diet of the patients is = 0.55; or calculated on the animal food 0·34 of full diet; at Hanwell it is on the same basis, 0·53 and 0·13; at Nottingham, 0·82 and 0·34."—Pp. 17, 18.

In all the asylums, with the exception of one or two, the patients are more or less employed in the house or in gardens and fields out of doors. Occupation in the open air is generally allowed to be most beneficial. At Hanwell, of 604 patients, 391 were employed; at Nottingham 68 out of 103; and at Stafford 110 out of 199.

"Some persons presume that because a man is insane he must be confined. Nothing can be more erroneous. It was shown, from the poor law commissioners' returns (p. 5), that of the total class, idiotic and lunatic, 15 per cent. only were confined in asylums; and that of the lunatic, exclusive of idiots, 43 per cent., were in asylums. This result is confirmed by a parliamentary return of pauper lunatics and idiots (1830), distinguishing the dangerous and not dangerous, the confined and the free. In 20 counties (first in the alphabetical arrangement) 3489 were returned; 1915 idiots, 1574 lunatics; 620 dangerous, 2867 not dangerous; 663 as confined, 2823 as free. Of 100 idiots and lunatics together, 82 were not *dangerous*; 18 were dangerous: of the lunatics, 40 per cent. were considered dangerous, 42 per cent. were confined. In Middlesex, 824 were returned; 652 as lunatics, 172 as idiots; 344 dangerous, 596 confined. The great proportion of lunatics, not dangerous, under confinement, is striking; more than half of them would be free in the country." P. 18.

According to Mr Farr, there are not more than 123 keepers in the 13 county asylums, or about 1 to 17 patients. At Hanwell, in 1835, there were only 22 keepers to 580 patients, or 1 to 26.

"There is usually a matron at each establishment; and the resident medical officer appears to act as housekeeper, house-steward, or general superintendent. According to the Act of parliament, 2 and 3 William IV. c. 107), when a private asylum contains 100 patients, there must be a resident medical officer; but it could scarcely have been contemplated that the resident medical officer should also be the *economico* of the Italian Hospitals. In 13 county asylums, there appear to be 11 resident medical officers—1 to 180 or 190 patients under treatment; in all 24 visiting and resident officers performing active duties—or about 1 to 88 patients resident. The offices of *consulting* physicians and surgeons are honorary, and cannot be taken into account. At Hanwell, attendance on 580 patients devolved on one physician and one surgeon; there was one medical officer to 290 patients; there was one-third less medical attendance at Hanwell than in the other county asylums. Primâ facie, few will hesitate to pronounce that two medical officers,—one charged with the onerous economical management of the establishment, while the other has to compound the drugs—are totally inadequate to the efficient treatment of 600 insane patients, or even half the number. The duties of steward or master (or whatever he may be termed), as they are totally different in their nature, so ought they to be entirely separated from those of the physicians. The mental qualifications and habits which fit a man for the one class of duties are not such as would qualify him for the other. The physician should have nothing to attend to but his medical duties. For the Hanwell Asylum, containing as it does 600 patients, a physician whose whole attention is devoted to the treatment of the patients, with two visiting assistants, one for the male and the other for the female department, and a third to compound the medicines and assist his colleagues, is the least amount of medical attendance capable of doing the duty efficiently; and it may be confidently predicted, that until such an arrangement is adopted, the result of the medical treatment at Hanwell will not bear a comparison with that of similar, but better appointed establishments."—P. 19.

Respecting the expences of lunatic asylums and patients, the following facts have been ascertained by Mr Farr. Twelve asylums cost L. 419,170 Sterling, the expense of each thus costing nearly L. 35,000. On Hanwell, L. 124,440 were expended. At Hanwell, L. 221 were sunk for the accommodation of one patient, at the other asylums, L. 201. Thus the relative expence did not decrease with the magnitude of the building. L. 2, 6s. is annually required for each individual for furniture and repairs. If the highest number of lunatics in the asylums, in 1833, was 2339, it is not probable that the average population exceeded 2100." For every individual, therefore, of the average population of the asylums L. 258 were sunk in furniture and building. In Ireland, 10 asylums cost L. 251,583, paid by county presentments. The number of beds was 1337. Each, therefore, costs L. 188.

" The weekly charge for pauper patients varied from 4s. 9d. to 10s. in the county asylums. At Gloucester, the yearly sum paid for the maintenance and treatment of a lunatic amounted to L. 26; at Norfolk, to L. 12, 7s.; the latter sum ' including salaries, and every other charge, except the repairs of the building.' On referring to the table of diet, it will be observed that the differences in the charge are only partially accounted for in that article. Patients from counties that have not been rated for the erection of asylums, are received at the Bedford, Cheshire, Cornwall, and Stafford asylums; these generally pay 12s. a week; and where a considerable number are received at this rate, the profits must be considerable. The charge to their own paupers at Stafford is very low (5s. a week), when the dietary is taken into account; but of 187 patients, only 98 belong to this district; 32 from other counties, pay 12s. a week; the rest are charity, or private patients paying from 12s. to L. 2, 2s. weekly. It was stipulated in the act of 1808, that the weekly charge to counties that built asylums, should not exceed 14s.; in the subsequent acts, the justices were authorized to exceed this charge if it were found insufficient. How little was known of the economy of these institutions, even in 1828, when the last act was passed! A building of L. 200, and 14s. a week to be devoted to each pauper lunatic! The charge for patients at the asylums has progressively decreased. In the Cheshire asylum, 1829-32, it was 7s. 6d.; in 1833-4, it was 6s.; in 1835, it was 5s. 3d.; and now it is 4s. 8d. a week. At Hanwell, the weekly charge was 9s., 8s., 6s. 5d.; now it is 5s. 10d. At Stafford, 1831-4, it was 6s.; in 1835, it was 5s.; at the Suffolk asylum it was reduced from 6s. 10d. (1831) to 5s. 8d. (1835). The weekly charge at the York W. R. Asylum has declined gradually from 10s. 6d. (1819) to 6s. 6d. If the actual average charge at the county asylums was 7s. on each patient, the annual cost to the counties was L. 18, 4s. + the annual value of L. 258 sunk in building, which at 7 per cent. will amount to L. 18. The actual weekly cost of each patient is therefore, 14s.; and it is this sum that must be compared with the charges at other places. From a return (Parl. Papers, 1830) of pauper lunatics (1829) before Hanwell asylum came into operation, it appears that 15s. was the highest, 5s. the lowest, 10s. 3d. the average sum paid by the parishes in Middlesex for paupers in confinement. Now if the annual value of L. 124,440, sunk in building, be assumed to be L. 8711, this sum added to the expenditure in 1835 makes L. 20256, which, divided by 580, the average number of patients, gives L. 34, 18s. as the annual cost of each to the county. L. 34, 18s. a year is equal to 13s. 5d. a week. From the return of 1829, before referred to, (Parl. Papers 1830-31,) it appears that for 596 lunatics, in confinement, L. 291; for 111, not in confinement, L. 42, 16s. were paid weekly in Middlesex.

Taking all the counties, the parishes paid as low as a shilling for patients not confined, and as high as 15s. for patients in confinement. The average expense of both classes varied in different counties from 2s. 6d. to 12s. 9d.; the mean in all the counties, except Middlesex, was 5s. 4d. Warwickshire had no county asylum; of 298 insane persons, 30 at county asylums cost in the year L. 988; 27 at private mad-houses, L. 839; 118 in workhouses, L. 1623; 122 not in confinement, L. 1188; 1 in a public hospital, L. 19, 6s.; the weekly charge per patient, at the different places, was 12s., 11s. 11d., 5s. 3½d., 3s. 8¾d., 7s. 5d.

" In Ireland, the average annual expenditure, per patient, was L. 20, 17s. (Mr D. Phelan on Irish Charities); the physicians to each had L. 100 a year; the manager had L. 200 a year; the matron, L. 50. At Hanwell, Sir W. Ellis, as physician and manager, has L. 500 a year!! the matroness, L. 200 a year, while the house-surgeon has but L. 100 a year." Pp. 21, 22.

Respecting criminal lunatics little is known, except that there is an annual vote of L. 2000 or L. 3000 for their maintenance. In 1835, the charge of confining and maintaining this class was estimated at L. 2590. A building for them was attached to Bethlem Hospital, and it was calculated in the estimates for 1835, that each confined there would cost 10s. 6d. per week, besides which three keepers were to be allowed L. 75 each for taking care of 30 patients. In addition to the money sunk in buildings, each criminal lunatic costs 13s. 6d. a-week, while at Stafford, a pauper lunatic costs only 5s. In 1835, the average number of criminal lunatics in Bethlem was about 52. If, therefore, L. 2590 were to be expended on them, each would cost the country L. 49, 16s. annually.

We pass over the remainder of Mr Farr's remarks on the public management of lunatics, on the irregular, careless, and unscientific mode in which statistical returns are moved for and made up, and on the expenses of the public inspection of lunatic asylums, because, without transcribing them, and thus extending this article beyond all ordinary limits, we could not place them satisfactorily before our readers. We conclude this necessarily imperfect view of the work with a statement of the author's views respecting the reform which he conceives ought to be made in the public management of the insane.

The necessity of a searching inquiry into the management of county asylums has been already admitted in the House of Commons. The inquiry, however, should extend to the management of the insane over the whole country, in whatever establishments they may be placed; for though it is impossible that the present unsatisfactory state of matters can continue, it is necessary to obtain several facts and authentic data before again proceeding to legislate. In the present stage of the inquiry, Mr Farr suggests that the management of lunatics should be divided into two absolutely distinct sections,—one to conduct the economical management, and another to direct the medical and hygienic treatment, or to register and determine its results.

Mr Farr farther suggests that, in the event of any alteration in the present arrangements, the economical management might be placed in the hands of the poor-law guardians, or the body proposed by Mr Hume to be elected by the payers of the county rate.

The estates of rich idiots and lunatics might be let to different individuals, on leases dependent on the expected duration of the attack, the rents paid into the Treasury, and, after the deduction of a certain discount, invested into the funds.

" With regard to attendance on lunatic asylums, a medical man should invariably reside on the spot ; he should see the patients every day, examine and direct their hygienic treatment, and prescribe the medicines required.

" The medical inspection should be invested in one body ; it should extend to all establishments irrespectively, every lunatic in the country should be included in the annual tabular reports."—P. 37.

The office and responsibility of inspection should be restored to the medical profession.

" *Five Medical inspectors* may be continued in the metropolitan district ; one to inspect quarterly each of four divisions, the fifth to classify the result of their observations ; the inspectors to visit the four divisions in rotation. Government may appoint five persons to visit and act with the medical inspectors ; or fifteen visitors as at present, who should have notice of the inspector's movements, be at liberty to accompany him, have access to the books, and be present when licences were granted. The fifteen visitors would share none of the responsibility ; they would form an audience ; they would exercise a silent control on the conduct of the responsible inspectors, and secure some of the advantages of publicity, where, as in private houses, full publicity would not be possible. * * * The licences should be granted, as at present, by the commissioners ; the money be received by the clerk, and deposited immediately in the Bank of England ; the accounts audited annually. One medical commissioner would be sufficient to inspect and licence houses in two to four counties, assisted by a clerk to fill up licences ; and accompanied by visitors nominated annually by the local authorities.

" Lunatics might be confined in urgent cases on the certificates of two medical men ; but, as a general rule, they should be brought before the local medical Board of Health, and examined on entering and leaving the asylum. The order for confinement ought in every case to be given by some responsible judicial authority, and be justified by the medical certificate, stating not merely that the patient was a lunatic, but a lunatic requiring confinement.

"With this machinery all the advantages of public inspection might be obtained, as well as an accurate series of statistical returns."—P. 38.

In order that an insane patient may receive the greatest degree of comfort, and have the best chance of recovery, it is necessary that those having the charge of him should have a direct interest in the prolongation of his life, and his restoration to sound mind. The ancient practice of giving the custody of a *natural idiot* to individuals who enjoyed his estate as long as he lived, was calculated to secure the unfortunate being all the comfort and happiness of which he was susceptible. Every possible care was taken of him, because on his death the estate passed from the hands of his guardian to his heirs. Mr Farr thinks this the best plan of proceeding, provided it was so far modified that only a part of the patient's income should be disposed of in this way.

In lunacy another element must be taken into account; the persons under whose care the patient is placed must be made to have a direct interest not only in his life but in his recovery. This might be effected by making them insure the lunatic's life and recovery to a variable amount, depending on the sum paid annually for his maintenance and treatment. The following examples will show the working of the plan.

"*Example* 1.—At Bethlem 6 persons die, 46 recover, in *one year* out of 100 admitted; if, therefore, *a pound* were deposited with each person at the time of admission, on condition that L. 16, 13s. should be paid to the relatives in case of death, this would leave a small surplus and the interest in the hands of the governors. For 100 persons would deposit L. 100; on an average 6 of them would die, and their friends would receive six sums of L. 16, 13s. each, in all L. 99, 18s. Observe how in this way the managers of the hospital would be interested in the lives of the patients. If 5 instead of 6 died, they would gain L. 16, 13s.; if 7 instead of 6 they would lose L. 16, 13s., a difference the slowest understanding could perceive. Again, if the governors deposited one pound in the hands of the patient's friends, on condition of receiving back L. 2, 3s. 6d. if he recovered during the year, they would be repaid; for 46 recover, and 46 times L. 2, 3s. 6d. = L. 100, the amount deposited by 100 patients. If only 23 recovered they would lose L. 50; if 69 recovered they would gain L. 50. They would be intimately interested in the patient's recovery. Without any deposit on either hand, then, it would only be necessary that the managers of the hospital should pay L. 16, 13s. when a lunatic died, and receive L. 2, 3s. 6d. when he recovered, to insure the greatest efforts possible on their part to effect recovery and preserve life. This was the problem to be solved. The medical officers and keepers should be in the same way interested in the patient's life and recovery."—P. 40.

The rates of payment on death or recovery would necessarily vary with the *age* of the patient, the *period* of the disease, its *complication* with epilepsy or paralysis, and previous attacks.

The chief difficulty to be anticipated is in determining the reality of the recovery. All disputed cases should be decided publicly by the commissioners, according to accurate definitions, and the tests should always be of a practical nature.

In taking leave of Mr Farr's work, it is but justice to state, that it contains more matter, for its size, than any volume we are acquainted with. Every possible sacrifice has been made to secure the greatest amount of information within the smallest compass, and thus in forty-six closely printed pages, materials have been accumulated, which an experienced book-maker would have found no difficulty in expanding into a goodly volume. To the statistical inquirer it is invaluable, and must prove useful to all who turn their attention to the subject of insanity.

We shall now briefly notice Dr Crowther's work. In the introduction we are presented with some severe remarks on the direction of asylums by the unpaid magistracy. In the second chapter he endeavours to show that insanity cannot be treated in the most efficient manner by one who devotes himself to that subject exclusively. The proposition is self-evident, and did not require so elaborate a demonstration as it has received from the hands of the author. No one would be foolish enough to declare that a man who devotes himself to the treatment of insanity, to the total exclusion of all knowledge of every other disease, is the best calculated for securing the recovery of the insane. All that has been averred is, that a well-educated physician, who devotes himself not exclusively, but principally to the study of the disease, is better qualified for taking charge of the insane than one who is engaged in general practice. The truth is, that a man in active practice cannot bestow the necessary attention on lunatic cases, and how few even of well-educated physicians do we find, who are competent to take charge of the insane. This must continue to be the case till some means can be devised to give clinical instruction in this as in other diseases.

The third chapter is a short tirade against quackery; the fourth shows that, wherever dysentery spreads in a lunatic asylum, it is owing to the negligence of the medical superintendent; the fifth details some instances of gross carelessness and neglect in the management of asylums; and the sixth and seventh chapters are devoted to the exposure of the means by which the number of cures is made to appear greater than it really is. In the eighth chapter Dr Crowther proposes his plan for preventing abuses in lunatic asylums. It is exceedingly simple, and consists merely in making the physician, surgeon, apothecary, superintendent, and matron spies on each other, examining and cross-examining them separately and secretly several times a year. The plan is wonderfully well calculated for rendering a retreat for the insane a pandemonium in miniature, and putting an end to all unity of action. The following is the passage to which we allude.

> " To prevent the disgusting scenes that have formerly occurred in mad-houses, all the superior officers ought, on their appointment, to engage to communicate to the governors every impropriety of conduct which falls under their observation; and for this purpose, the physicians, surgeons, apothecary, clerk, director, and matron, being all separate and independent appointments, ought to be examined by the governors separately, several times a-year."—Pp. 43, 44.

We could not have conceived that any member of the profession could have deliberately proposed a plan so reprehensible and absurd. We consider it reprehensible, because, morally speaking, no scene can be more unbecoming than that of the officers and attendants of an establishment, for the management of which a rare combination of skill, intellect, and moral power is required, incited to act towards each other in a way which would rouse the indignation and contempt of the lowest of mankind. It is absurd, because no man possessing the qualifications requisite for the responsible situation of medical superintendent of an asylum would, for a moment, submit to a system of espionage, misinterpretation and secret *ex parte* judgments, which might have the effect of blighting his prospects in life.

In the ninth and tenth chapters, Dr Crowther points out the evils attending the election of superintendents by the visiting magistrates, and recommends that all the officers connected with asylums should be placed under a minister of health, assisted by a medical board, who could judge adequately of the merits of candidates for vacant situations.

The next four chapters are devoted to remarks on the qualifications and duties of the physician, surgeon, and superintendent of an asylum, which, though containing nothing new, are judicious enough. In the fifteenth chapter, the author points out what he conceives to be the best means of securing the constant and efficient attendance of the superintendent. These are to pay him moderately, to leave him to support himself out of his salary in an adjoining house, instead of living and boarding in the asylum, to give him plenty of work, and to have at the disposal of the medical board, situations of different degrees of responsibility and emolument, for the purpose of rewarding merit. One very important matter he seems to overlook, and that is, to take care to choose a man whose natural taste and habits lead him to cultivate with enthusiasm this department of medicine. Such an individual would require none of those galling and cumbrous checks which Dr Crowther thinks so necessary.

The sixteenth chapter contains the author's views of the qualifications and duties of the matron of an asylum. He objects, and as we think, very properly, to the situation being filled by the wife of the superintendent, but he appears to be haunted with the notion, that the superintendent and matron of an establishment must necessarily be irresistibly impelled to do evil, and he has recourse again to the plan proposed before, of making them spies on each other. "In institutions," he observes, "so liable to be abused as mad-houses, it is much preferable to have the offices of director and matron filled by individuals unconnected and independent of each other, who may be examined separately, and made instrumental in discovering each others delinquencies."

In the seventeenth and eighteenth chapters, he protests against the fitness of magistrates to govern asylums, and recommends the appointment of local visitors as in Glasgow, Manchester, Birmingham, and Leeds. The remainder of the volume is devoted to matters of a miscellaneous nature, the more important of which will be found discussed in Mr Farr's work.

We cannot conclude this brief notice of Dr Crowther's work, without expressing our regret that he should have allowed his personal feelings to be so mixed up with the subjects under discussion, We suspect that the disagreeable disputes in which he was some years ago involved with the visiting justices of his district, as well as dislike to, or prejudice against, Sir W. Ellis, and medical superintendents in general, have influenced him too much on the present occasion. Throughout the whole book there is to be found a display of censoriousness which, though amusing enough, is utterly absurd.

ART. V.—*Report on the Malignant Fever called the Pali Plague, which has prevailed in some parts of Rajpootana since the month of July* 1836. *Prepared and published by order of the Government of India, under the direction of the Bengal Medical Board.* By JAMES RANKEN, M. D., Officiating Secretary to the Board. Calcutta, 1838. 8vo. Pp. 232.

PALI or Palee is a town and district in the territory of Merwar or Marwar in Upper India, not far from the tributaries of the Loonie, a river which runs westward, and falls into the Runn, or large salt marsh of Cutch, communicating with the Bay of Cutch. The town of

Pali stands on the north bank of one of the tributaries of the Loonie, in north latitude 25° 50′, and 73½° east longitude. The district of Pali is intersected by numerous streams, which descend from the western side of a hilly ridge, extending north-east and south-west, denominated the Mhairwarah mountains, and which forms the eastern boundary, not only of the district of Pali, but of the territory of Marwar, and the extensive country of Rajpootana. It appears that this mountainous ridge forms the most elevated part of Upper India, since most of the tributaries of the Loonie, which flows west and falls into the Runn of Cutch, issue from their western margin, while several of the tributaries of the Bunnas, the great western branch of the Ganges, descend from the eastern side of this range.

The district of Pali is in the principality of Marwar or Joudpore, which is the capital of this principality, a town or city as it is often named, of considerable size and population, situate on an eminence a little to the west of the Jajree or Meckree river,—the largest tributary of the Loonie.

It is difficult to ascertain the exact amount of the population of the town of Pali. It is represented to be not under 15,000, and according to the vague mode in which such statements are often made, it is said to be about 15,000 or 20,000 souls. The town itself is reckoned the emporium of the trade carried on between central India and the sea-ports of Guzzerat.

This district was originally occupied by the Rajpoots, who are described as a bold, hardy, and robust race of people, delighting in military exercises and warlike pursuits. Whether, however, from poverty, from their hereditary pride preventing them from pursuing agriculture, or any other species of industry,—or from having carried on a long system of defensive warfare against the Pindarries, they are said to be indigent, rude, dissolute and abandoned to an extreme degree, stained with every vice, sensualists, and above all devoted to the use of opium and spirits, to the extent of almost habitual intoxication.

The surface of Rajpootana in Jeypore, Oudeypore, and part of Joudpore, is a succession of sandy vallies interspersed with ranges of low rocky hills, in which the rain is arrested as it falls at irregular periods, and forms by its descent the multiplied streams by which the district is traversed. The soil is represented to be clayey. It is evident that it does not readily part with moisture; for the whole surface abounds in marshes during the cold and rainy season; and in ordinary years, by digging, water is abundantly found near the surface. The whole country, indeed, may be represented as an elevated table land, surmounted by the Mhairwarah hills, the rain and dews descending from which, form the inexhaustible sources from which the rivers flowing to the west into the bay of Cutch, and to the east into that of Bengal, issue.

The produce of the country is represented to be chiefly reeds, an evident proof of its humidity, brush-wood, and stunted trees. Neither grass, nor herbage, nor stately forests are said to embellish its surface ; yet to the hand of industry it is said not to be barren. Notwithstanding its humidity, however, it is said to be liable to frequent droughts, which again are succeeded by famine and disease. In the year 1833, a destructive epidemic, and in 1834 fever, raged among the villages.

In 1835 no report of any extraordinary sickness in this district was received.

In the month of July 1836, however, there appeared in Pali a febrile disorder remarkable for the great rapidity of its progress, the mortality which it caused, and the peculiar symptoms with which it was accompanied. It was denominated by the natives the Waba or Wuba, the Murree or Mari, and the Idubu, and at Náthdwára the Gant Ki Mándagi.

The disease first appeared among a number of families, amounting to about 2000 persons called *Chipahs*, resident in Pali, and who procure their subsistence by printing the pieces of plain cloth brought from the west; and of these *Chipahs* 665 died. The Brahmans next, then the Mahajans or retail merchants, and at length the inhabitants indiscriminately, were successively taken ill. Of the fifteen or twenty thousand persons believed to form the population of Pali, 4000 perished, at the rate of 50 or 60 daily. Before the mortality abated, all the wood which could be procured for burning the dead was exhausted; and latterly the corpses were consumed with the shells of cocoa-nuts, and the butter commonly used as food. The survivors of their kindred, and most of those who were not early attacked, speedily deserted the town, and sought refuge in the neighbouring villages. But the inhabitants often refused to receive them. It is further stated, that many of the persons who left Pali in this manner were at length admitted into the houses of their friends; and that wherever they took up their abode the fever shortly afterwards appeared. It is not mentioned whether those who fled or those who received the fugitives were first attacked.

A few places are said, on uncertain authority, by avoiding all communication with the town first sickly, to have continued healthy. As the season proceeded, nevertheless, all the villages, amounting to twenty-four, within a circuit of thirty miles from Pali, speedily began to suffer from the distemper which had first appeared there. In September the epidemic had extended to Soojit, a town under the 26° N. L. containing 6000 souls ; and by October it had crossed the Loonie and Jairee rivers, and reached Joudpore, the capital of Marwar.

Hitherto its progress had been nearly at equal rates northward, westward, and eastward, at nearly equal rates to the base of the hilly district of Mhairwarah. But it then appeared in this district in the village of Dewair, where it destroyed 400 persons, passed over the hilly tract to Deoghur, in Meywar, whence it passed to the north-east by Loosaneh, Jannee, and Bednore to Jalia and Ramghur, in the district of Aymeer,—during January, February, and March 1837. In April the appearance of the sickness was announced at Bhilwarah and Humeerghur, towards the British cantonment of Neemuch, on the road between that post and the military station of Nusseerabad.

In the district of Meywar, it eventually affected not fewer than thirty-two villages, most of them situate along the sources and tributaries of the Bunass.

The mortality was certainly very great; but it is impossible to obtain accurate statements. As in all great public calamities, the loss was exaggerated by the panic-struck inhabitants, who, in May 1837, believed that it had destroyed 100,000 persons. Lieut.-Colonel Alves, Political Agent in Rajpootana, reckoned the deaths in Marwar at 20,000, including 6000 in the city of Joudpore; and at the same time Lieut.-Colonel Speirs, Political Agent in Meywar, computed the number of dead in that principality at 7022. According to the concurring testimony of those medical officers who saw and treated the disease, not more than one-third of those attacked recovered.

It was a general opinion at an early period of the appearance of this disorder, among the medical officers who saw this disease in the inhabitants of Pali, that it was true plague, and contagious in origin and propagation; and it was deemed expedient by the public authorities to adopt measures calculated to prevent the further propagation of the disease. The Bombay Government, within the limits of whose territory a similar distemper had prevailed in 1821, was the first to adopt precautionary measures; and in August 1836, when the sickness had already continued rather more than one month, orders were issued to establish a preventive line of posts, intersecting all the commercial and commonly travelled roads from Beanmahul to Balmere, which being guarded by horsemen and troops, were expected to subject the communication between Pali and the Bombay territory to complete control. Medical officers, also stationed near the most frequented highways, had instructions to detain all goods and persons from suspected places, until, after the employment of effectual means of disinfection, they could furnish certificates equivalent to clean bills of health, allowing them to pass the quarantine posts.

With this example and the opinion of professional advisers before him, the Lieutenant-Governor, Sir Charles Metcalf, conceiving that immediate danger impended over the north-western provinces, issued a minute, with instructions to his subordinate officers in what manner to act, in order to impede and prevent the approach of the apprehended pestilence, and to mitigate its severity in places where it had already appeared.

Sir Charles Metcalf in this proceeding adopted the notion, that the disease was true plague; and, with the example of Sir Thomas Maitland before him, he drew up a series of instructions as to non-intercourse and seclusion, precisely in the spirit and letter of these celebrated orders. These regulations, which consist of forty-three separate paragraphs, and are given in the Appendix to the present Report, are too lengthened to be here reproduced. But we may say in general, that Sir Charles Metcalf assumes the disease to be true plague, and decidedly contagious in origin and propagation,—avows that the sentiments and intentions expressed in the minute are founded on the information contained in Mr Tully's work on the Plague in the Mediterranean Islands, corroborated by Dr Hennen's published sentiments on the same subject, and by other authorities; and declares his intention to reprint for circulation and general information the work of Mr Tully, and also the Reports on the Pali Disease given by Messrs Maclean, Keir, and Irvine, all of whom declared the disease to be true plague, and decidedly contagious. A cordon or preventive line was accordingly drawn round the infected and sickly district; and the usual destruction of dogs, cats, and other domestic animals was recommended, in order to remove, as completely as human means could effect, all modes of conveying the suspected material cause of the distemper.

> " Orders were given to isolate every town or village in our territory as soon as it became affected; and to allot six separate places within it to the following purposes:—1. An hospital for the infected; 2. a depot for the strongly suspected; 3. another depot for the slightly suspected; 4. a place of quarantine for new comers; 5. a receptacle for infected or suspected goods, where they might be deposited until purified; and, 6. a place of residence for expurgators. These expurgators, conveying the sick to the hospital and the dead to their graves, disinfecting their houses and destroying tainted articles, were to perform all the duties expected of persons bearing that designation in the east of Europe. The line of circumvallation, guarded partly or wholly by armed men, to prevent ingress or egress, was to allow no provisions to enter unless by authority of the quarantine officers; and in paying for them, the inhabitants of the infected spot were to drop their money into a cup of water, whence the sellers might take it without risk."

It thus appears that the infected district of Pali was subjected to as rigorous a system of non-intercourse and seclusion as is practised in any of the ports or situations on the Levant in which quarantine regulations are observed and enforced.

It is not unimportant to advert to the proceedings adopted after the disease appeared in Pali and the adjoining villages, to prevent the hilly district of Mhairwarah from becoming infected. This district, which is 90 miles in length, and varies from 25 to 30 in breadth, is situate between Marwar and Meywar, forming a sort of neutral territory between the two principalities, claimed by the sovereigns of both, and is now held in trust by the British Government, under the management of Captain Dixon as superintendent. As the distemper appeared first in the western or Marwar side of Mhairwarah, the superintendent taking two watchmen from each village, placed them in a continuous chain along the Marwar frontier of Mhairwarah. It does not appear, nevertheless, that this line prevented the disease from passing over this hilly tract; for it was found requisite to establish, though the time is not specified, another line along the eastern or Meywar frontier of this hilly range. The sickness, nevertheless, appeared in Meywar in the regular course; and its progress appears neither to have been rendered less rapid, less steady, or less destructive, by the establishment of these sanitary lines. Meywar, therefore, became infected in numerous points, thirty-two villages at least, as already stated; and it became an object of importance to prevent the disease from spreading out of this district northward; so on the 11th April 1837, according to the official despatches of Captain Dixon, a third line was drawn in a direction eastward and slightly south, from Loolooah, in the Mhairwarah hills, to Sukranee, in Meywar; and this line was afterwards continued in the same direction by Lieutenant-Colonel Alves to Deoleah and Sawor, on the Bunass river; and as a line already extended along the south of this principality to the hilly country between the rivers Bunass and Chumbul, the wretched territory of Meywar was completely insolated, inclosed, and secluded, as far as human means could effect it on three sides; the fourth, directed to the rugged hilly district already mentioned, being supposed of less moment.

It was natural to suppose that precautions so complete would effectually prevent the disease from appearing beyond the guarded barrier. This, however, was a mistake. The distemper, apparently as if in defiance of these guarded lines, appeared in Ajmere and Nusseerabad, on the north of the lines, and in Neemuch, on the south.

The epidemic, however, declined progressively after the commencement of the hot dry season in the month of April 1837. It disappeared and again returned in many parts of Marwar, and it seems to have undergone the same sort of spontaneous subsidence and revival in several places in Meywar.

The symptoms of the Pali distemper were those common to intense remitting or remitting-continuous fevers in India, with the addition of painful enlargement of the lymphatic glands, terminating in suppuration, if life continued.

The approach of the disorder was in general sudden, without previous feeling of indisposition. The patient began to shiver sometimes slightly, to have pain in the head, back, and loins; and speedily the pulse was quick, from 130 to 150, but soft and compressible, seldom full and bounding. According to Dr Irvine, however, the pulse was full and strong, about 110 at first, and afterwards it was small, soft, and very rapid. The skin became very hot and dry; sickness followed, and sometimes vomiting. At the same time the tongue was covered with a fur white and moist, or light-brown and dry; and in a few cases it is said to have been nearly in the natural state. Considerable thirst was a common symptom. The eyes were heavy, maudlin, or turbid, and bloodshot or injected, not unfrequently of a sort of lake-colour. The countenance was expressive of anxiety and internal pain. The abdomen was generally tumid and hard, but without sense of pain on pressure, and in the early stage of the disease the bowels were bound. The breathing was in general not much disordered, unless in some patients, in whom there were symptoms of congestion or inflammation of the lungs, and in whom hemorrhage from these organs usually took place.

In the majority of the severe cases the feebleness was very great.

Glandular swellings appeared in the groins, arm-pit, and neck, usually on the left side, in some cases almost simultaneously with the first symptoms of fever, but in the majority of cases in the course of the first or second day; and in a few cases so late as the third or fourth day. They were at first small, movable, and always acutely painful to the touch. In a few cases they rapidly increased in size, suppurated and discharged purulent matter, either alone or mixed with shreds of dead cellular tissue. In the far greater number of instances, however, they did not become larger than the bulk of a walnut, and showed no disposition to suppurate. These buboes were most frequently seen in the groins. In some cases one appeared in each groin, in some one in one groin and one arm-pit, in some, in one or both arm-pits, in others, again, in one or both arm-pits and in the neck, in one or both groins, and in the neck, and in some on the neck alone.

Suppuration and rapid increase of size were observed by the Pali inhabitants to be favourable symptoms. But this result was not necessary to the favourable termination; and in many persons who recovered, the buboes are stated to have often gradually disappeared of their own accord. In one person seen by Mr Maclean, a bubo in the left groin had attained so great a size as to extend from the pubis to near the anterior-superior space of the ileum, and was very hard and painful; yet the fever had ceased four days before he was seen by Mr Maclean.

In the Pali distemper all the symptoms underwent a remission, more or less marked, towards morning, the remission being shorter or longer in duration according to the mildness or severity of the disorder in each case. In the most intense cases, however, no remission was observed.

In a few cases the disease was so mild that patients walked without assistance from their houses to the residence of the medical attendant, had their buboes examined, the pulse felt, and medicine administered, and walked home again. In others, however, so great was the feebleness, that fainting followed any attempt to raise them from their charpoys.

Though the disease always commenced with acute pain in the head and orbits, according to Mr Maclean the intellectual functions were rarely affected; and most of the persons he saw answered questions readily and distinctly. Dr Irvine again states that with the lake-coloured turbid aspect of the eyes, delirium ensues; Dr Keir mentions cases in which the patients spoke incoherently, though when roused and addressed in a loud voice, he gave intelligent replies; and Mr T. Russell states that he saw at Nathdwara three delirious cases, and one comatose In fatal cases, indeed, coma preceded death for a few hours.

In a small section of the cases of the Pali disease, symptoms of inflammation of the lungs appeared on the first or second day of the disease. The patient complained of acute pain of one or both sides, or beneath the sternum; great difficulty of breathing, and had short dry cough; and usually on the second or third, a small quantity, about half an ounce, of florid blood, in small clots, was expectorated. Though buboes occasionally appeared on the individuals labouring under the pulmonic symptoms, yet it was more common for buboes not to be observed, The mortality in the pulmonic cases was so great, that a person on seeing blood in his expectoration gave himself up for lost.

Such is the delineation of the usual character of this distemper. In some situations it has presented a considerable variety in the symptoms.

At Jodhpore, for instance, the capital of the district, where thousands died of the epidemic, in very few or none of the cases were buboes observed; and hence the disorder there was said not to be the plague, and to have no connection with the distemper which first appeared in the town of Pali. (Dr Irvine's Letter, April 4, 1837, No. 8.)

At Ramgurh also, there were no cases of hemorrhage from the lungs.

The most important point in this inquiry now is to determine the nosological character of this distemper, to ascertain its nature, whether contagious or void of such a property, and to discover the causes on which its presence and operation seemed to depend.

I. We have already said that almost all the medical officers who were directed officially to report on this disease, gave it as their opinion that it was merely inguinal or glandular plague, or the plague of Egypt and the Levant. Such was the opinion of Mr H. Maclean, Dr R. H. Irvine, Dr Keir, and several others. This opinion was founded on the symptoms of the disorder, the rapidity of their progress to the fatal termination, its great mortality, and, above all, the appearance of glandular swellings on the groin, armpits, and neck. Perhaps it might be argued that it rested also on the contagious manner in which they and the inhabitants believed the disease to be propagated. But as this would be to assume what ought to be proved, and to give in evidence of a conclusion, a mere opinion, which rests on a very slender and unsubstantial foundation, we must decline, in the meantime, receiving this as part of the evidence.

This opinion, though received by the political and military authorities, and made, as we have seen, the basis of all the practical measures, has by no means received such ready credence elsewhere. Thus it appears that Mr Thomas Russell, in his letter from Nathdwara, of the 20th Nbvember 1837, could not satisfy himself of the transition of the disease from one person to another, and was inclined to regard whatever sickness he witnessed or heard of as the effects of inflammation, not of contagious or putrid fever; and he further stated in a letter of the 28th November, that he had heard of no instance of one individual being infected from another in that town, and that at Kankrowlee, which was also represented to be the seat of the Pali sickness, the inhabitants treated with contempt all the preventive precautions of the British officers. Mr Roger Foley, also, who visited Kotah, a place of 3000 inhabitants, in April 1837, states in his letter of the 25th April, that the accounts of the disease were much exaggerated; that several persons had drank cold water while overheated, and had been attacked by inflammation of the lungs, which had proved fatal from want of assistance; that at the village of Barau, containing about 14,000 inhabitants, were crowds suffering under ague, for which they solicited the means of relief; that, in the surrounding villages, the inhabitants were suffering from fever, attributed to the fatigue of attending the celebration of their religious festivals; and that the whole district was liable to intermittent fever, while no trace of the Pali disease was observed. Mr A. Maclean, also, assistant-surgeon to the 9th Cavalry at Jaipur, represents intermittent and remittent to be very prevalent in that place, yet that he was unable to discover proofs of the existence of the Plague, that is to say, Pali disease; yet at this very time (15th May —19, 1837,) the average amount of deaths was 18 daily, and on the 18th 24 had died.

These testimonies, it may be argued, are all of doubtful weight. But we have now to add that Dr Ranken, the author of the present report, has, after a careful comparison of the phenomena of this disease, with those represented by the best authorities, to distinguish the presence of Plague, openly avowed his belief, "that the Pali disease is not the Plague of Egypt or of the Mediterranean, imported from abroad, and propagated by specific contagion." His reasons are the following:

1. All the best authorities, as Russell and other observers, represent as pathognomonic in oriental Plague, carbuncles, wheals, or (stripes rather,) vibices, spots, (the tokens) and lividity of parts after death. These appearances, however, were never met with in the sickness of Rajpootana.

2. The affection of the lymphatic glands is the only symptom common to oriental plague and the Pali sickness. These glandular enlargements, however, are so far from being pathognomonic, that, in many instances of true Carbuncular Plague, they do not appear at all; and, conversely, glandular enlargements attend malignant fevers of every type in all climates. Thus Cleghorn describes glandular enlargements and suppurations in the bad tertians of Minorca. Rush observed them in the yellow fever of Philadelphia. They have been often seen in the continuous and remittent fevers of the West Indies. And every one knows that they are frequently seen, as they were by Drs Barker and Cheyne, in typhous fever in Europe.

3. It appears that fever with similar glandular enlargements, and evincing equal malignity of type and mortality, has prevailed several times already in various of the East Indian territories. Thus it appears that a febrile disorder in all respects similar prevailed in Bombay in 1818. Of this we shall probably speak more fully afterwards. Further, at the time at which the glandular febrile distemper was raging in Marwar and Meywar, a febrile disorder of extreme malignity in the intensity of its symptoms of prostration, the violence of the delirium, the occurrence of hemorrhage, the sudden progress to coma and death, raged like a pestilence at Moradabad, and destroyed at least two-thirds of those attacked. This disease prevailed both in the jail and in the town of Moradabad, from April 1836 to the middle of October, attacking principally those living in confined, badly ventilated situations, and where the population was dense. In many cases it terminat-

ed in yellowness of the surface. A distemper in all respects similar prevailed soon after at Barcilly, Coel, and in the territory round Delhi. This epidemic Dr Ranken regards as in all essential circumstances similar to that which received at Pali and in Marwar the formidable name of plague. It further appears, that in the mountainous district of Kamaon, a very malignant fever, rapid in progress, and extremely mortal, accompanied also with glandular swellings and suppurations, has prevailed at intervals during the last fifteen years, and was particularly fatal in the years 1834 and 1835.

From all the facts now mentioned, combined with the circumstance of the physical characters of the country and the habits of the inhabitants, we think it impossible to conclude, that the Pali distemper was any thing like true plague, or that it was any other than the ordinary remittent or continuous-remittent, endemial in the country. It is certain that endemic fever is there exceedingly common, and is indeed the genuine product of the country and climate; and every one who knows the great variety of symptoms which this disease in certain seasons, and under particular circumstances assumes, will not be surprised to find, among these symptoms, the phenomena of enlarged painful and suppurating glands. All the observers agree in the fact, that the fever presented a remittent type; it is impossible to deny that remittents become under certain circumstances of soil, season, atmosphere, and weather, most fatal and malignant disorders; and, why the mere circumstances of glandular enlargement should have made the medical officers employed to observe the disease, and report on its nature, to refer it to the head of the plague of the Levant, when no carbuncles were visible, is an anomaly which we must own our inability to understand.

While we have been preparing the account of the Report of Dr Ranken now submitted, we have received from our respected correspondent, Dr Morehead, the first volume of the Transactions of the Medical and Physical Society of Bombay, which contains, among other interesting materials, three papers on a febrile disorder of singular malignity, which has prevailed in various parts of that presidency, in the years 1815, 1816, 1817, 1818, 1819, and 1820;* and from the accounts there given respectively, by Mr White, Mr M'Adam, and Mr Gilder, it is impossible to doubt, that, so far as similarity in symptoms and fatality goes, the disease which prevailed in Cutch and Kattywar in 1815 and 1816, in Kattywar and the Zillah of Ahmedabad in 1817, 1818, 1819, and 1820, was quite the same as that which in 1836 appeared at Pali, and spread over the principalities of Marwar and Meywar. Our limits will not permit us to enter into the history of these epidemics, as given in the reports now referred to; but when we do so, which we trust shall be at no distant period, we think it will be possible to show that it is quite the same as the Pali distemper, and that it was, like it, not the carbuncular plague, but merely the indigenous remittent of the country.

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* A Report of a Disease which prevailed in Kattywar, and in parts of the Zillah of Ahmedabad, in the years 1819 and 1820. By Thomas Whyte, Esq. formerly of the Bombay Service.—A Report on a Disease which prevailed in part of Cutch and Kattywar, in the years 1815 and 1816. By J. M'Adam, Esq. Superintending Surgeon, Deccan Division, formerly Civil Surgeon in Kattywar.—A Report on a Disease which prevailed in part of the Zillah of Ahmedabad, in the years 1817, 1818, and 1819. By G. J. Gilder, Esq. formerly of the Bombay Service.—Transactions of the Medical and Physical Society of Bombay, Vol. 1. Bombay, 1838. 8vo.

II. With regard to the question of imported or domestic origin when applied to the Pali and Rajpootana distemper, our readers may partly conjecture the views which we take, from what has been already stated as to the character of the disease. An attempt appears to have been made, as similar attempts have in similar circumstances been made, to trace the Pali distemper to foreign origin, and to fix the fact of its importation upon Zerawar Mull, a great banker, loan-contractor, and merchant of Rajpootana, who, having gone to Guzzerat on business, it was alleged, returned with merchandise which had come from Bhaonuggur or Surat, and stopped several days at Pali, just before the first appearance of the sickness. It was then inferred that, as these sea-ports receive goods from the Persian Gulf, the Red Sea, and from Egypt itself, the native soil of pestilence, Zerawar Mull must have brought the plague by that means to Pali. It was ascertained, however, that this personage had proceeded, not on a commercial, but a religious pilgrimage to the temple at Teeruth; that he neither bought nor sold on his journey; that he returned in good health; that no importation of any articles from pestilential countries had taken place; that no unusual sickness prevailed either at Bhaonuggur or Surat, or in the provinces therewith connected; and that the foreign merchandise purchased in these markets, for many years past, of Western, Northern, and Central India, were not even suspected of communicating diseases anywhere except at Pali.

When this hypothesis of the origin of the distemper was shown to be void of foundation, it was again asserted, that numerous small traders, availing themselves of the escort of Zerawar Mull, came in his train to the emporium of Marwar, and, though in perfect health themselves, they might have purchased at Bhaonuggur unopened bales of Egyptian cloth, and disposed of them to unsuspecting retail merchants at Pali, who being the first to unpack the tainted bales, would be the first to suffer from the foreign contagion. There is reason to believe, Dr Ranken states, that the only cloths received at the period referred to were English cottons;

but while the original purchasers must have escaped at the opening of the packages, the printers who received the pieces to embellish were the earliest victims of the fever. The truth is, the whole of this hypothesis was forced, gratuitous, and void of foundation, and required a degree of imagination beyond the most extravagant fictions ever framed. To these Dr Ranken justly gives no credence; but he is mistaken in supposing, that "so long a chain of improbabilities has seldom been devised to support an untenable hypothesis." If he will recal to recollection the extraordinary and fanciful stories which were related, in order to account for the appearance of yellow fever in Grenada and the West India Islands in 1793, when a disease which is well known to be the natural product of the West Indies was represented to have been imported from the Island of Bulam on the West African coast,—if he will recal the stories told to account for the appearance of yellow fever in Spain, in 1800, 1804, 1820, and 1828,—he will readily admit, that equally improbable coincidences were then imagined to have taken place, in order to make that appear to be imported which, certainly, was of domestic origin.

If to what has been already said by Dr Ranken upon this point, it were requisite to add other arguments, we would say, that sufficient evidence is collected to prove that the soil and climate of India, with the habits and modes of life of the natives, are of themselves quite adequate to the production of such fevers as the Pali distemper. It is, therefore, quite unnecessary to have recourse to foreign agents to explain the origin of that which all the known facts concur in showing to be of domestic growth. It is always agreeable to the mind, and is often very natural, amidst the consternation and panic produced by great public calamities, such as general epidemic sickness, to think that it has not proceeded from among ourselves, and that it has been introduced from some other quarter. But when the mind has recovered from the panic with which it was struck, and when observation and reflection are permitted to resume their sway, both the existence and operation of foreign agents become doubtful, and the inquiry is suggested, whether all the phenomena might not have been produced as readily by the influence of agents much nearer home. In the present case, there was, and could be at Bhaonuggur or Surat, and other places in Bombay, nothing which did not exist in Rajpootana; the same physical surface of country, the same climate, the same atmospherical conditions, and the same kind of inhabitants, living in the same manner, or perhaps in a condition far more favourable for the developement of domestic or home-born disease.

III. With regard to contagious propagation from one person to another, or by means of contact, direct or indirect, with the body, or the inhalation of the breath of a patient, Dr Ranken finds no evidence to prove that this properly characterized the Pali epidemic, or any fever hitherto known in India.

Dr Ranken even maintains, that while in genuine plague, though little or no danger seems to result from breathing the air of the same apartment in which a pest-patient is kept, yet the slightest contact is very liable to be followed by the disease,—this peculiarity was completely wanting in the history of the Pali and Moradabad fever. None of the medical officers or their servants, who handled patients affected with the Pali disease, and felt their pulses for days and weeks, presented any symptom of the disorder. He thinks, however, that while any person who kept his hand a whole day in contact with one suffering under the Pali or Moradabad fever, if he continued to breathe pure air, would not be attacked by the disease,—he would very likely suffer from it if he sat within the same hut, and exhaled the same atmosphere for half the time. He believes, in short, that the disease is communicable by the lungs through the atmosphere, but not by contact.

It is, in short, the opinion of Dr Ranken, that though fevers of this kind are the genuine and unequivocal product of the soil, yet they are in part produced by the filthy habits, the crowded and ill-ventilated abodes, and the miserable and scanty supplies of food of the inhabitants, who are, he says, in a state of extreme indigence and destitution.

" The people of the north-western provinces of British India, and of Marwar especially, are to be considered in varying stages of transition from anarchy and want to comparative plenty and order. They have advanced within reach of many improvements on the physical condition of society, but having made no corresponding acquisitions in intelligence, their minds retain the impress of former barbarity and misrule, and are yet unable to turn the advantages of a better system to account. Hence animal instinct, predominating over foresight and enterprize, population has augmented faster than increasing agriculture and commerce have extended. The hut, or one of the same dimensions, which was the unwholesome den of four persons, for example fifty years ago, now shelters six human beings within a space barely sufficient for the accommodation of one. These miserable habitations are often enclosed by an outer screen or wall, for the double purpose of confining the cattle and secluding females from the public gaze. Glass to admit light, or apertures to admit fresh air being unknown, the door, in order to keep out heat at one season, and cold at another, is generally shut. The poor feel most at home in such dark places, with their children lying round them on the floor, too much like hogs in a sty amidst their litter. The sense of insecurity of life and property, which deterred their ancestors from living near the fields which they cultivated, in farm houses and cottages, scattered over the face of the country, owing to hereditary habits of thinking, still perceptible to this day, under the very cannon of Fort William, makes the peasantry everywhere accumulate their hovels within the narrowest limits, and the consequence is, that each dirty village, unventilated and over-crowded with men and beasts, exhibiting every sort of nastiness, is a focus of disease."—Pp. 35, 36.

This picture is not overcharged. The testimony of all the surgeons officially employed concurs to show that the villages and towns of Upper India, and the north-west provinces, are so densely peopled, and that their inhabitants are in such a state of indigence and misery, that to the innate powers of the soil and climate in the production of disease, are united the additional influence of concentrated human and animal effluvia, under an equinoctial sun.

"Whatever may have been the original cause," says Mr Shirreff, "I have no hesitation in saying, that the epidemic has assumed great part of its fatal character from the foul, crowded, or ill-ventilated state of the villages and houses which it has attacked. To particularize one is needless. In every village where I have found the disease (yellow fever) raging with severity, filth and want of ventilation were remarkable. The villages of Bhow in the Rhotuck, and of Quaribpoor in the Paniput district, might perhaps, on a superficial examination, be excepted; but on entering the dwelling-houses of those suffering most severely, the eye and nose bore immediate testimony to the necessity of classing them among the others. Farridpur (Paniput District,) which suffered severely, is a low crowded village, surrounded by the usual mounds of ordure and filth of every kind, besides having at one end a half-dried tank. Ventilation, so necessary under such circumstances, is all but absolutely prevented by high ruinous walls, and by a surrounding grove of palm, mangoe, and other trees. In the village of Pakism (Rhotuck district,) where the disease was still raging with peculiar severity when I visited it, might be seen numerous houses in which living beings, sick and healthy, were crowded into a space barely sufficient to contain them. I counted in the interior of one hovel of the ordinary size, twelve buffaloes, besides some calves and a pony, all but pressing on individuals labouring under different stages of the epidemic. Four of these sufferers were lying in two beds, and a fifth after some time was discovered breathing his last in a place like a baker's oven. The pent-up putrid vapour, for it was not air, of this place nearly suffocated me, no attention having been paid to the commonest cleanliness. Seven individuals had already died out of this from the epidemic; many other houses in the same village were similarly circumstanced. In the village of Relloi, Rhotuck district, which suffered much from the fever before I could see several of the sufferers, I was obliged to break through herds of cattle, which occupied the only ventilated parts of the houses, and perhaps was let to the innermost of a suit of cells, where I found them in all the distress arising from excessive heat, disgusting filth, and an ardent fever; the patients here, and indeed in many other places, being invariably placed in the closest apartment, in order, as they said, that the perspiration might be encouraged."—P. 37, 38.

More of these and similar proofs, our limits will not permit us to adduce. It is sufficient to say, that while the general sources of terrestrial miasm or malaria are universal, various causes of local influence, too manifest to be overlooked, are specified.

It is rather curious to observe the slender grounds upon which the contagious character of the Pali distemper was admitted. Several proofs of the slight evidence required by the visiting surgeons could be adduced. But the following, which answers the purpose sufficiently well, is an average specimen.

"The disease is eminently contagious at Jalia. I will instance a few cases. In a Brahman's family, six members died within seven days, all previously living and sleeping together. Another old brahmani woman was attacked; she recovered; during her recovery her son and grandfather were attacked, and both died. They all lived and slept close together. A *baniya* constantly nursed his son (12 years of age,) in his arms till he died; the father was then attacked, and died in a very depressed state in twenty-four hours; and many similar cases. I am of opinion that the disease is highly contagious."—P. 35. Dr Irvine's letter of 11th March 1837.

Now, instead of recognizing in these details proofs of contagion, we think they might be made to demonstrate the fact, that all the cases proceeded from one common cause. Assuredly the fact of six persons under the same roof, dying of the same disease within seven days, cannot be said to indicate the operation of contagion; and certainly no contagious disease ever proceeds to attack simultaneously so many persons living together. In every contagious disease there must be a latent period, or a period of incubation, that is, a time or interval between the period of exposure to the alleged cause, and the evident or manifest production of its effects on the person of the individual or individuals exposed. The average duration of the Pali disease is from three to five days. When the first person of this family was attacked, he must have been ill at least two or three days; and supposing the period of incubation to be two or three days, and that he or she was capable of communicating the disease on the second day of its duration, at least other two days, probably three, must have elapsed before the fatal result took place. This would occupy at least five days, and at most seven, before a second death could take place, according to the hypothesis, that one person in this family infected the other. The fact, however, is, that the whole six persons died within a space of seven days; and consequently the sickness of each must have either succeeded with the greatest rapidity,—too great a degree of rapidity to allow of the interposition of a latent period,—or several must have sickened at the same time,—which amounts equally to the same general result. There is, therefore, no way of accounting for the rapid succession, or simultaneous occurrence of the sickness and mortality of such cases as these, except by supposing that the whole six persons had received the alleged contagious poison from one source, or by supposing that the sickness had originated from one common cause. The former supposition is exceedingly improbable. The latter, therefore, is the only alternative.

In the other cases mentioned by this gentleman, we think it not less clear that all originated from one common cause; and that cause does not resemble in its properties or effects what is denominated contagion.

One of the most decisive proofs that the disease was internal or domestic in origin, and not imported, is found in the fact, that all the measures of seclusion and non-intercourse which were adopted, were inadequate to prevent its dissemination over the whole of the principality of Marwar, in every direction indiscriminately and also across the Mhairwarah ridge into that of Meywar.

That the disease was named Plague, however, is to us not matter of wonder; and the same mistakes have been committed by other observers, under the influence of the novelty and alarm at the sudden invasion, overwhelming energy, and unsparing mortality of an epidemic distemper. The term Plague has been indeed applied so generally to every disease of which the approach is sudden, the symptoms intense, the progress rapid, and the mortality considerable, that it is not wonderful that, in the loose and vague manner in which various professional persons not unfrequently think and express themselves, this epidemic has also been designated plague. Thus, in the time of Oliver Cromwell, the yellow fever of Jamaica, which destroyed in a few weeks 2000 men among 7000 of the force under Venables in 1677, was denominated the plague, and whenever the plague is mentioned in the West Indies or America, at that period of history, it is invariably the yellow fever or endemial remittent, that is meant. Captain Dover also, afterwards Dr Dover, who, when he sailed with Captain Woodes Rogers and Captain Courtney, in the capacity of buccanier, was present at the capture of Guiaquil, applies the name of plague to the endemic fever with which a large proportion of the crews both of the Duke and Duchess were attacked His account deserves being generally known.

"When I took by storm the two cities of Guiaquil under the Line, in the south seas, it happened that not long before the plague had raged among them. For our better security, therefore, and keeping our people together, we lay in the churches, and likewise brought thither the plunder of the cities. We were very much annoyed with the smell of dead bodies. These bodies could hardly be said to be buried; for the Spaniards abroad use no coffins, but throw *several dead bodies* one upon another, with only a draw-board over them; so that it is no wonder that we received the infection.

"In a few days after we got on board, one of the surgeons came to me, to acquaint me, that several of my men were taken after a violent manner, with that languor of spirits that they were not able to move. I immediately went among them, and to my great surprise soon discerned what was the matter. In less than forty-eight hours we had in our several ships 180 men in this miserable condition."

The treatment and its effects deserve attention.

"I ordered the surgeons to bleed them in both arms, and to go round to them all, with command to leave them bleeding till all was blooded, and then come and tie them up in their turns. Thus they lay bleeding and fainting so long, that I could not conceive they could lose less than 100 ounces each man.

"If we had lost so great a number of our people, the poor remains must have infallibly perished. I begun to consider that the plague was no more than fever of the most exalted kind; and so *duro huic nodo durior adhuc cuneus adhibendus.* We had on board oil and spirit of vitriol sufficient, which I caused to be mixed with water to the acidity of a lemon, and made them drink very freely of it; so that, notwithstanding we had 180 odd down in this fatal distemper, yet we lost no more than seven or eight; and even these owed their deaths to the strong liquors which their messmates procured for them." *

It is certain, in the *first* place, that no genuine plague was ever known in South America; *secondly*, that the disease of the country is the remittent in various degrees of intensity; and *thirdly*, no attack of plague was ever known to cause, within so short a space of time as forty-eight hours, the sickness of 180 persons.

Lastly, an anonymous author, in the first volume of the Transactions for the Improvement of Medical and Chirurgical Knowledge, p. 53, describes, under the name of plague, a case of remittent fever, and gives some general notices of the course of the disease, which was then (1780) epidemic at Bussorah on the Euphrates. In this instance the disease was very intense, sometimes very rapid in progress, and very fatal. It was computed to have destroyed 25,000 persons between the end of May and the middle of October.

IV. On the propriety of the system of quarantine regulations and restrictions it is not our intention to say much. If the views of Dr Ranken and those which we have taken be well-founded, these measures were in all senses unnecessary and inadequate, and in some detrimental. They were unnecessary, in so far as it was clearly proved that the disease never affected any of the English practitioners or their native servants; inadequate, because they did not prevent the sickness from spreading over the whole of Merwar and Meywar; and detrimental, in so far as the restrictions imposed on intercourse and commerce proved the source of much distress and suffering in many parts of both principalities. The only political or military governor who appears to have understood the grievances of this system was Lieutenant Macnaghten, resident at Ajmere, who, in consequence of a petition from the merchants and traders of that city, complaining of the oppressive effect of the quarantine regulations, and the enormous loss occasioned by unpacking and examining carelessly and hastily the bales of cotton; the want of uniformity in the system; and various abuses attending its execution; remonstrated strongly indeed, but rationally, against its continuance. We regret to observe, that the sensible and humane recommendations of this gentleman were not only disregarded, but that he appears to have been treated not in the most respectful manner,—a circumstance, which, as the course pursued by the government was more than doubtful, reflects little credit on those who so acted. It seems hardly possible, indeed, for greater evils to have taken place under the most free and unrestrained communication than those which ensued under the regulations of the quarantine; and, had the agents of government been instructed to explain to the panic-struck natives the necessity of spreading themselves over more extensive spaces of ground, and observing complete cleanliness in their persons, clothing, houses, and mode of managing their cattle, we are confident that they would have rendered much more effectual service to this ignorant and deluded people, than by enforcing all the rules of the most rigid quarantine.

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* The Ancient Physician's Legacy. By Thomas Dover, M. B. London, 1733, p. 80.

Of treatment it is scarcely requisite to speak. Every method that was tried was unavailing. The English surgeons proposed the use of blood-letting with cathartics. But to the former the native Hakims were entirely averse; and when medicines were given, they were often left untaken. It may be doubted, however, whether in the treatment of a disease of such intensity of symptoms and rapidity of progress, any remedial measures can be admissible or useful. As in all severe epidemics, when the disease commences, it runs its course. The only effectual remedies against the Pali disease must be of the preventive kind, or those which remove the causes and counteract their operation; by teaching the inhabitants the necessity of cleanliness, and of living in larger and better aired abodes.

We cannot close this article without observing, that the present report reflects great credit on Dr Ranken. It is drawn up in a cool, temperate, and rational manner; the facts are fully stated without any attempt at explaining them; the conclusions are judiciously drawn; and, as it required some strength of mind, to avoid being carried away by the force of the prevailing opinions, the author has shown that he possesses this quality in a high degree.

ART. VI.—*Chemistry of Organic Bodies. Vegetables.* By THOMAS THOMSON, M. D., Regius Professor of Chemistry in the University of Glasgow, F. R. S. L. and E. &c. London, Paris, Leipsic, 1838. 8vo. Pp. 1076.

THE knowledge of the chemical constitution of organic bodies is to the physiologist and physician a subject of the highest importance. The physiological inquirer can scarcely move a single step without the aid of accurate chemical analysis; and it is well known how much the progress of physiology, both animal and vegetable, has been in modern times beholden to the facts disclosed by chemistry, as to the constitution of the parts of animal and vegetable bodies. To the physician a competent share of this kind of knowledge, and that of a very accurate description, is not less important and useful. All the articles of food and drink are obtained from the bodies of animals and plants, or are preparations from these substances; and a large proportion of the agents employed as remedies in the cure or alleviation of disease is obtained from the vegetable kingdom. And as it is difficult, if not impossible to comprehend the principles, on which different kinds of food or drink are selected, or different remedial agents are chosen, and the modes in which they are most conveniently prepared, combined and administered,—it becomes a duty incumbent on every intelligent physician to be well acquainted with the chemical constitution of the articles either of the Dietetic Table or of the Materia Medica.

The investigation of the chemical constitution of animal and vegetable bodies, however, has been always a task of extreme difficulty; and the discordant results at which different chemists of acknowledged talent and undoubted accuracy have arrived, have served to show the arduous nature of the undertaking, and have consequently roused the ambition of the most able and distinguished analytical inquirers.

Some of this difficulty has arisen from the fluctuating and changeable state of organic bodies; since many of the parts both of animals and plants possess a different chemical constitution at different periods of their living existence. Others have proceeded from the complicated nature of the structure of organic bodies, and the multiplicity of simple substances which enter into their composition. A third source of difficulty has arisen from the extreme facility with which their principles have been decomposed, and made, in the very process of analysis, to enter into new combinations. From all these circumstances, and others which might yet be enumerated, it has resulted that the chemical knowledge of organic bodies has been the least perfect and the least satisfactory department of chemical science. Ten years ago, indeed, it might be said to be only in its infancy so far as precise information goes;

and we have daily occasion to see errors rectified, and deficiencies supplied, in regard to subjects in which it was hastily believed that information was complete and accurate.

Of late, however, the analytical examination of animal and vegetable bodies has been conducted with such zeal and industry, and with such improved means in insuring accurate results, by a great number of able chemists in different countries of Europe, that hopes may be entertained that the subject will at length assume the appearance and attributes of an exact science. These researches are at present scattered through so many different works, transactions of scientific bodies, scientific journals and other publications, that it is of the greatest consequence to the physiological and medical reader to have the whole presented to his view in one work of reasonable dimensions.

The present publication promises fair, in this respect, to realize the most sanguine expectations. Composed by one who has spent his whole life in the practical researches of the laboratory, who has from the first bestowed particular attention on the history of the chemical composition of organized bodies, and who has been always engaged in observing the progress of chemical discovery in Europe, it possesses all those advantages which are likely to render it an instructive work, to those who require full and accurate information on the subject of organic chemistry.

The present volume comprehends the chemical history of vegetable bodies, and the author gives full accounts of all the analyses performed by the ablest and most accurate chemists.

The arrangement of the work may be understood from the following sketch.

Dr Thomson makes four great divisions; the first, treating of vegetable principles; the second, of the parts of plants; the third, of vegetation; and, the fourth, of the decomposition of plants.

Under the head of vegetable principles he gives the chemical history of, 1. the vegetable acids; 2. the vegetable alkalies; 3. intermediate bodies, as alcohol, ether, colouring matters of different kinds, oils, fixed and volatile, and resins; and, 4. neutral vegetable principles.

In the second division the author gives an instructive account of the sap and peculiar juices; the gases of plants; the chemical history of the different kinds of barks, the roots, bulbs, various sorts of wood, the leaves, the flowers, the seeds and fruits; then the cryptogamous plants, and, lastly, the diseases of plants.

The third division contains an interesting account of the phenomena of vegetation, its several processes, and its final effects. And, in the fourth and last division, under the head of decomposition, is given a history of the different kinds of fermentation, and the various products to which they give rise.

The work may be recommended as the most comprehensive and useful system of vegetable chemistry extant.

PART III.

MEDICAL INTELLIGENCE.

MEDICAL THERAPEUTICS.

On External Pressure in Prolapsus Uteri. By **John F. Gray, M.D.** of New York. (From the London Medical Gazette, November 10, 1835.)—Prolapsus uteri has two essentially distinct forms, one of which has not been sufficiently noticed in our manuals of female diseases; I allude to a dislocation of the womb downward and slightly backward, the os tincæ tending toward the coccyx. The ano-perineal region of Velpeau, or the perineum posticum of the older anatomists, from relaxation of the levator and sphincter ani, becomes enlarged, and the triangular space between the point of the coccyx and the tuberosities of the ischian bones forms, in consequence, a broad deep *cul-de-sac*, into which the uterus sinks in the line of its own axis, and rests against the anus and rectum.

This posterior dislocation of the womb often takes place in pregnancy, particularly during the first four months: but it also takes place under other circumstances, I have no doubt, and that much more frequently than is commonly supposed.

Sir Charles Clarke[*] speaks of the uterus descending against the perineum, and there resting as upon a shelf. I have seen very many cases of this kind, in which there were vaginal discharges of various kinds, frequent false calls to go to stool, vesical irritation, and dragging pains in the lower part of the back and groins, from the utero-sacral and round ligaments being put upon the stretch.

Indeed, I believe nearly every case of confirmed fluor albus, not connected with the prolapsus of the books, is accompanied by this form of uterine displacement. If from an uncommon curvature of the sacrum backward, a large pelvis, increased volume of the uterus, relaxation of the ligaments, or any other cause, the ano-perineal region may be large enough to receive the impregnated womb as late as the seventh month,[†] and if retroversion of that organ does frequently take place after labour and after miscarriage, why should there not be procidentia or delapsus uteri in that direction as frequently at least as in the other? There exists, as far as I am aware, no anatomical impediment to the posterior dislocation, and the uterus, under the depressing forces of the abdomen, as in the act of defecation, must, it should seem, always tend toward the perineum posticum; and if the utero-sacral ligaments are somewhat more relaxed than the recto-vaginal septum, the fundus uteri would fall forward, and the whole viscus sink downward and backward in the line named, *i. e.* the axis of the superior strait of the pelvis.

The posterior displacement will be readily recognised by examination per anum. The finger will have to pass very much more backward than usual, to get around the os tincæ, which lies hard against the rectum, just above the sphincter ani, and is very perceptible to the feel of the surgeon.

In passing the finger per vaginam, the neck of the womb is first encountered, occupying the situation of the os uteri.

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[*] Observations on Diseases of Females, 2d edition, page 62.

[†] Vide Madame Boivin on Disease of the Uterus, p. 73.

The os uteri is found lying against the rectum, its aspect being backward and downward toward the point of the os coccygis, and the finger will have to be carried back in a curved form to reach it. The space between the os uteri and the posterior termination of the vagina appears much larger than natural.

In addition to the ordinary symptoms of prolapsus, the patient experiences a sensation, from the pressure of the *os uteri* against the rectum, of a foreign body lying in the intestine just above the anus, and feels as if she ought to void it by going to stool,—a peculiar tenesmus, the attempt to obey which, of course, only aggravates its cause. Besides this sensation, many varieties of leucorrhœa are dependent upon the posterior displacement of the womb. In fact, discharges from the vagina not traceable to syphilitic taint, and not arising from scirrhus or polypus uteri, are very apt to arise from this cause, and are to be remedied by the application of "Hull's utero-abdominal supporter," an external force which will be found to replace the prolapsed uterus with great facility, whether it be of the anterior or posterior species. This posterior dislocation of the womb may very readily be mistaken for internal piles, *prolapsus ani*, or even stricture of the rectum, and be accordingly maltreated.

Thousands of females afflicted with false disposition to go to stool, inveterate fluor albus, occasional menorrhagia, painful or irregular menstruation, urinary difficulties, dyspeptic symptoms, and anomalous sufferings in the back, loins, and pelvis, have the misfortune to be treated for these symptoms as so many separate maladies, whose cases arise from the posterior prolapsus, and might, by aid of Dr Hull's apparatus for the external cure of *prolapsus uteri*, be speedily alleviated, with a fair prospect of radical restoration to health.

This subject should be carefully attended to in every case. The diagnosis is certainly not difficult, when assisted, or, as I may say, determined, by examination both per anum and per vaginam; and when it is fairly established that the womb has sunk back and down upon or into the *ano-perineal* region, the application of Dr Hull's supporter ought not to be delayed.

The pessary in this case does no good whatever. It is thrust into the ano-perineal region, already rendered a sac by relaxation and by the presence of the dislodged womb, and there as a really foreign body, excites the same sensations and keeps up the same irritation and discharges which the womb had done, and generally as may readily be supposed, the latter are of a very aggravated character; whereas the new instrument of Dr Hull, by pressing the ano-perineal region upward and inward, directly opposes the descent of the womb, and at the same time diminishes the capacity of this region, whilst the hypogastric support of the apparatus prevents the descent of the abdominal viscera into the pelvis in a manner I shall hereafter notice. The cup-and-ball pessary of Bauhin, with its spiral spring improvement by Recamier, is not much better than the ordinary pessaries. It presses upwards and backwards towards the promontory of the sacrum, and not upwards and forwards in the axis of the superior strait, and therefore does not meet the uterus in a direction opposite its line of descent, but meets it an obtuse angle. I have nothing to add to the ordinary descriptions of prolapsus, but that they only regard those dislocations of the uterus in which its fundus falls downward and backward, and the os uteri traverses the vagina, appearing first at the vulva in complete prolapsus, and presenting first to the finger, in the incomplete or procidentia, and that in so far these descriptions are totally imperfect, excepting the remark quoted from Sir Charles Clarke, speaking of the kind I have described.

In reference to the causes of the various prolapses of the uterus with the accompanying displacements of the bladder and rectum, I beg leave to state my dissent from the received notions of relaxation of the ligaments of the uterus, and the weight of that viscus being the cause of the displacement.

In the dead body, the uterus cannot be pushed downward, even after an entire division of its ligaments.† And the vagina, which Dr Dewees supposes to be the main support of the uterus, is always a flexible collapsed passage, except during the moments of orgastic turgescence. It is, besides, very frequently relaxed, and even inverted, without being accompanied by displacement of the womb. It seems to me really absurd to suppose the round ligaments, which are considerably curved, and therefore loose (not "tort,") with the duplicatures made by the peritoneum about the fundus uteri, which are certainly far from appearing like real ligaments, would support the womb against the enormous depressing force of the diaphragm and abdominal muscles. if the ano-perineal muscles and aponeuroses were removed; *i. e.* if the ano-perineal were divided and reflected back, or dissected entirely away. That nature never intended these falsely called ligaments to keep the uterus *in situ*, is evident from their presenting no resistance whatever to the ascent of that viscus in gestation. Mr Pott‡ speaking of the proximate cause of hernia, says, "If the cavity of the abdomen is always full completely, the containing and contained parts react upon and reciprocally compress each other. It is by the effect of this moderate, but equal and unremitting pressure, that all the viscera mutually support each other. Without it the ligaments of the liver, those of the spleen and the various membranous bands of the intestines in general, would only be feeble means for fixing such parts in their respective situations.

"But there are certain points of the abdominal parietes which naturally present much less resistance than others. Such is, particularly, the part which extends from the pubes to the anterior superior spinous process of the ilium. This relative weakness of some points of the abdominal parietes is very marked in certain individuals, in consequence of a defect of organization. It may also be increased by internal or external causes. The conjoined powers of the abdominal muscles, diaphragm, and levator ani, are then directed and concentrated against the most feeble point of the abdomen, towards which they impel the nearest viscus, or that which from its moveableness is the most liable to displacement."

Thus an equilibrium in the containing muscular forces of the abdomen is fairly shewn to be more essential to the relative positions of the viscera than so called ligaments are. But Mr Pott has not alluded to the important and very obvious antagonism between the muscles of the perineum and the diaphragm, and oblique muscles of the abdomen—a consideration of the greatest consequence in accounting for prolapses of the uterus, or hernia of the pelvis. Velpeau, in his Surgical Anatomy, § makes the following forcible illustration of this view of the subject:—"The coccygei and levatores ani-muscles here form a species of septum of diaphragm, which is a perfect antagonist to the thoraco-abdominal septum." The coccygei levatores ani-transversales perinei, covered within by the aponeurotic fascia pelvia (Cloquet), and without by a strong subcutaneous layer or fascia, form a firm muscular septum, which closes the lower outlet of the pelvis, and which, from its moving with every respiratory act of the diaphragm, rising and falling in constant antagonism to the rise and fall of that septum, is certainly aptly called the lower diaphragm. I conceive *relaxation of this perineal apparatus,* whether caused by labours, coughing, lifting, &c., or by actual disease, weakening and attenuating its fibres, to be an essential condition to the existence of displacements of the pelvic viscera. The perineum, by which I mean all the parts enumerated, and they are so recognised by Velpeau and others, has to perform for a large space in the female, a resistance equal to very much of the depressing forces of the upper abdominal parietes: for the rest of the pelvis, being bone, lined with polished fascia, and of the form of an irregular inverted cone, reflects nearly all the forces they receive downward upon it; the only bony assistance it has is afforded by the coccyx, when fixed by its muscles and the levatores ani. If, then, this resistance be diminished, the perineal cavity must be increased, and the uterus must sink down, followed closely by the small intestines, propelled by the superior force of the upper muscles. A vacuum cannot certainly exist in the abdominal cavity; and as Mr Pott justly observes, the moment the equilibrium (of muscular power) is lost, a displacement of some of the viscera takes place. That the intestines are displaced, and closely press upon the falling uterus, is averred by Velpeau,* and if I am not mistaken, by Sir Astley Cooper. As a powerful remote cause of displacement of the pelvic viscera, relaxation of the broad tendinous terminations of the external oblique muscles should not be overlooked. These tendons counteract in a considerable degree against the downward forces of the abdomen, and relaxation in them produces a fulness in the iliac regions which very much favours the descent of the abdominal upon the pelvic viscera.

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* Doane's Good, Vol. ii. p. 472; Cooper's Dictionary; Mad. Boivin; Clarke, &c.

† Dewees' Diseases of Females.

‡ Pott's Works, Vol. ii.

§ Dr Sterling's translation, Vol. ii. p. 259.

I have seen several cases of distension of the inguinal regions very marked indeed, connected with dyspepsia, and premonitory of hernia, arising from relaxation of these broad tendons; but more generally in cases of descent of the intestines upon the pelvic viscera, the whole hypogastrium is enlarged, and the umbilical region flattened, and even hollows in accordingly.

I am convinced that more observation will detect this state of the abdomen in *prolapsus ani* and pelvic hernia, as well as in *prolapsus uteri.*

These views are ably set forth in a pamphlet addressed to the medical profession explanatory of Dr Hull's utero-abdominal supporter.†

"In cases of long standing," says Dr Hull, "the posterior portion of the vagina becomes very much distended, and the whole of the soft parts closing, the lower outlet of the bony pelvis assumes a pouch-like figure; and this unnatural cavity is filled by the (prolapsed) uterus and bladder, *held down by the displaced abdominal viscera.*"

If, then, the uterus be considered a passive body, and like most of the abdominal and pelvic viscera, held *in situ* by the equally balanced muscular, tendinous, and bony walls of the great cavity, and the correctness of the idea be admitted, that its displacement, at least in its unimpregnated state, is owing to relaxation of either the perineal or hypogastric portions of those walls, or of both,—the *rationale* of Hull's external support against these weakened and relaxed portions is quite evident, and the solution of its undeniable success ceases to be a surgical enigma. Dr Hull's apparatus has many advantages over the pessaries, which render it highly worthy the attention of the profession. In the first place, if there be scirrhous polypus, slight ulceration of the vagina or *os tincæ*, it is by no means injurious in its effects, but, as I have seen it in each of these cases, proves beneficial; perhaps more so than any and all other treatment: whereas pessaries of every kind are totally inadmissible, however urgent the coincident prolapsus may be, and the patient is necessarily confined to bed incessantly, with but a meagre hope of restitution. This advantage obviously relieves the practitioner of great anxiety in regard to the diagnosis, which in these cases is often painfully difficult, and to me not seldom wholly impracticable, especially in the first eight or nine years of my practice.

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* Ut supra, Vol. ii. p. 287.

† A Brief account of the Application and Uses of the Utero-Abdominal Supporter, &c.

2d. The evils produced by pessaries are wholly avoided; viz. the difficulties and unpleasantness of their proper introduction, frequent removal, cleansing, and replacement; and the inflammation and ulceration produced by them,* which are always tormenting, and sometimes fatal.

3d. The perineal wedge and strap of Hull's instrument, by pressing upon the whole median line of the ano-perineal region from the occcyx to the vulva, in an upward and forward direction, close the vagina from without, and tend to strengthen it, while its calibre is diminished; whilst the pessary of every kind distends the vagina, and tends to press it against the rectum and anus, producing many unpleasant sensations, and seldom if ever accompanied by salutary results.

4th. Hull's apparatus can *always* be applied in prolapsus, whereas it not unfrequently happens that inflammation or tenderness of the parts or incipient organic affections, prevents the introduction of pessaries entirely, and the patient is obliged to submit to confinement, strict regimen, and the like, for a very long time, which rarely fails of making lasting and serious inroads in her general health, even if the visceral displacement be removed.

MEDICAL EDUCATION.

Regulations to be observed by Candidates, for the Diploma of the Royal College of Surgeons of Edinburgh.—Surgeons' Hall, Edinburgh, 28th September 1838.—The Royal College of Surgeons of Edinburgh, in revising from time to time their regulations respecting the course of study to be followed by Candidates for their diploma, have been uniformly anxious to extend and improve the education of those who receive that testimonial of qualification, so as more and more to secure their being competent to perform their professional duties with credit to themselves and advantage to the public.

It will be obvious to all who consider the extended and complicated nature of Medical science, that much of the success of the student, in the prosecution of its various branches, must depend upon the previous cultivation of his mental faculties; and that it is consequently of the utmost importance, both as regards the interest of the public, and the future comfort and respectability of the practitioner, that all who apply to the study of Surgery should have previously received a liberal education. The College have enacted some regulatious having the securing of this for their object; and they confidently trust, that medical practitioners in every part of the country will be disposed to second their endeavours, by recommending to the young men who may be placed under their care, or who may apply to them for advice, the study of the *Latin, Greek,* and *Modern languges,* and of *Logic, Mathematics* and *Natural Philosophy,* as the best preparation for entering upon a course of medical and surgical education. And they would strongly urge their own Fellows, as well as all other practitioners, not to take any young man as an apprentice, until he shall have gone through such a preliminary course.

As the profession of surgery is a practical art, it must be obvious that skill in its exercise cannot be acquired without actual opportunities of becoming familiar with the phenomena of diseases, and with the effects of remedies. And although the College have not considered it advisable to prescribe the serving of an apprenticeship as an *indispensable* part of the education to be required of candidates for their diploma, they have no hesitation in expressing their conviction, that Students of Surgery cannot fail to derive much advantage from being placed under the superintendence of competent and conscientious masters, both as regards the prosecution of their professional studies, and the acquisition of practical skill in the treatment of diseases. The College strongly recommend to all masters, to give directions to their apprentices as to the classes which they ought to attend, and the books which they ought to read,—to subject them to occasional examination, with a view to ascertain their progress in the different branches of their education,—and to explain, as frequently and as fully as possible, the nature of the cases entrusted to their care, and the principles on which the treatment ought to be conducted.

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* See Mad. Boivin, Cooper, and many other writers on this subject.

The College have marked, with much satisfaction, a gradual increase in the attainments of those who present themselves for examination at Surgeons' Hall; and they are inclined to hope, that the regulations now enacted will tend to maintain, and to increase, the anxiety of candidates to raise their acquirements to the highest attainable standard. The College are at the same time aware, that their best efforts to improve the course of study will be incapable of effecting this desirable end, unless they shall be seconded by the influence of parents, guardians, masters, and teachers.

Chap. I.—Schools of Medicine; Qualifications of Teachers.—1. Every candidate for a Surgical diploma must have followed the course of study afterwards to be specified, in a University; or at the seat of an Established School of Medicine, as defined below; or in a Provincial School specially recognised by the College.

2. Under the title *Established School of Medicine,* are comprehended all places in this country where diplomas in Surgery are granted, and such Foreign Schools as are acknowledged by the constituted authorities of the countries in which they exist.

3. The extent and period of study allowed to be gone through at a Provincial School, will be regulated by the means and facility of study which the College receive evidence of its affording; but the Lectures delivered at a Provincial School will only be held as qualifying for one year's course of study, unless specially recognised for more.

4. *In Edinburgh,* the lectures to be attended as part of the course of study, shall be delivered by Professors in the University, by Resident Fellows of the Royal College of Physicians, or by Fellows of the Royal College of Surgeons; and *elsewhere,* by Professors of Universities, by Fellows of the Royal College of Physicians of Edinburgh, by Fellows or Licentiates of the Royal College of Physicians of London, and of the King's and Queen's College of Physicians of Ireland, by Fellows of the Royal College of Surgeons of Edinburgh, London, and Dublin, and of the Faculty of Physicians and Surgeons of Glasgow; and by persons holding a Medical Degree or Surgical Diploma, whose courses of lectures shall have been recognised by the College on special application.

5. The College will not recognise any course of lectures delivered subsequently to 1st May 1839, by a Professor or Teacher who lectures during the same session, upon more than one of the branches of instruction included in its curriculum; nor will it recognise any course of lectures delivered by a Professor or Teacher who, in addition to the said course, lectures upon a branch of instruction, medical or general, not included in the curriculum, unless such Professor or Teacher shall have obtained special leave from the College.

6. Notwithstanding the above regulations, the teaching of the following branches may be conjoined, without disqualification being incurred, viz.

—Anatomy and Practical Anatomy; Chemistry and Practical Chemistry; Practice of Medicine and Clinical Medicine; Practice of Surgery and Clinical Surgery; Mathematics and Mechanical Philosophy. And *for the present*, Clinical Medicine or Clinical Surgery may be taught in conjunction with any one of the other courses of education prescribed by the Curriculum, by a Physician or Surgeon, qualified according to the regulations of the College, and attached to a Public Hospital of the size which these regulations prescribe.*

Chap. II.—Course of Study.—1. *Preliminary Instruction.*—Every candidate for the diploma of the Royal College, must either previously to, or during his medical education, have received regular instructions in the *Elements of Mathematics;* and must have subsequently attended a course of *Mechanical Philosophy* of at least *three* months' duration, and of not fewer than sixty lectures, delivered by a Professor of that branch in a University, a Lecturer in a public institution, or a Teacher specially recognised by the College.

2. *Professional Instruction.*—The candidate must have been engaged in attending the following *separate* and *distinct* courses of lectures during a period of not less than twenty-seven months; in which must have been included three Winter Sessions, of six months' duration each.

		Duration at least.
Anatomy, - - -	2 Courses,	Six months each.
Practical Anatomy, - - -	-	Twelve months.
Chemistry, - - -	1 Do.	Six months.
Practical Chemistry, - - The number of Pupils in each Class being limited to 25, - -	1 Do.	Three months
Materia Medica and Pharmacy,	1 Do.	Six months.
Practical Pharmacy,† - -	1 Do.	Six months.
Institutions of Medicine, or Physiology,	1 Do.	Six months.
Practice of Medicine, -	1 Do.	Six months.
Clinical Medicine,‡ - -	1 Do. or 2 Courses,	Six months. Three months each, during the period of attendance at the Hospital where they are delivered.
Principles and Practice of Surgery,	2 Courses,	Six months each.
Or Principles and Practice of Surgery and Military Surgery,§	1 Course, 1 Do.	Six months each.
Clinical Surgery,‡ -	1 Course, or 2 Courses,	Six months. Three months each, during the period of attendance at the hospital where they are delivered.

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* In making the last-mentioned concession respecting Clinical Medicine and Surgery, the College wish it to be understood that this is done rather in consideration of the present state and arrangements of existing schools, than from their entertaining any doubt that the teaching of either of these branches would afford ample occupation for a teacher, in addition to his hospital duties.

† This is required of every candidate who has not served an apprenticeship to a regularly licensed Medical Practitioner, keeping a Laboratory for the dispensing of Medicines. It must be attended, *subsequently to attendance on Chemistry and Materia Medica*, at the Laboratory of a Surgeon or Apothecary; or of a Chemist and Druggist recognised by the College, on special application; or of a Public Hospital or Dispensary: and the candidate must produce evidence that he has been engaged in compounding and dispensing Medicines.

‡ Clinical Medicine and Clinical Surgery cannot be attended at the same time. In these courses, in addition to regular lectures delivered twice a-week, there must be a third weekly meeting for clinical instruction, which may be given in the way of examination, or conversation.

§ The course of Military Surgery must be delivered by a Professor of that branch in a University, or by a Lecturer who, in addition to the other required

Midwifery and diseases of Women and Children, . .	1 Course,	Three months.
Medical Jurisprudence, -	1 Do.	Three months.

3. The six months' courses delivered in *Edinburgh* must consist of not fewer than 110 lectures, with the exception of Clinical Medicine, Clinical Surgery, and Military Surgery. The three months' courses must consist of not fewer that 60 lectures. Two *London* courses of three months each, on any of the above subjects, shall be taken as equivalent to one six months' course.

4. The candidate must also have attended for twenty-one months a Public General Hospital, containing at least eighty beds.

5. The following order of study is recommended as a guide to the student, though not absolutely enjoined.

First Year.—Anatomy, Chemistry, Mechanical Philosophy, if not previously attended, Practical Chemistry, either in this or the second year.

Second Year.—Anatomy, Practical Anatomy, Institutions of Medicine or Physiology, Surgery, Materia Medica and Pharmacy, either in this or the third year, Hospital.

Third Year.—Practice of Physic, Clinical Surgery, Practical Anatomy, Practical Pharmacy, Hospital.

Fourth Year.—Surgery or Military Surgery, Midwifery and Diseases of Women and Children, Clinical Medicine, Hospital, Medical Jurisprudence.

6. The College strongly recommend to students to avail themselves of the opportunity which they may possess of attending lectures on Botany, Natural History, Comparative Anatomy, and Pathological Anatomy, in addition to the courses of lectures which are absolutely required by the above regulations.

Chap. III.—Registration Certificates.—1. A book shall be kept in the Hall of the College, for the Registration, at stated times, of all medical students who may apply. In this book all Edinburgh students who intend to apply for the diploma of the College, shall enter their names and the classes which they attend, whether under Professors in the University, or under other qualified teachers.

2. The registration shall be conducted by the Conservator of the Museum, or by a Substitute for whom he shall be responsible. He shall enter in separate columns, 1st, The name of each individual; 2d, The Medical Classes, Hospitals, and Dispensaries, attended by each during the current season; 3d, The names of his Teachers. He shall register such only as apply personally to him, shall examine all the Tickets produced by each individual, and shall not register any Classes for which Tickets are not produced.

3. At the close of each session, students shall produce to the Registrar the certificates of attendance they may have received from their respective Teachers (See Appendix); the terms of which shall be entered in the registration-book.

4. Each student shall pay annually, on his first registration for the year, the sum of five shillings, and shall be provided with a ticket of admission to the Museum.

5. The book shall be closed for the registration of the winter classes on the 30th of November in each year; and of the summer or intermediate classes at the end of the *second* week from their commencement.

6. When a student is prevented by sickness, or other unavoidable causes, from complying with the above regulations, he shall, *as soon as the case admits*, present a statement of the circumstances, with proper evidence, to the Conservator, to be laid by him before the Registration Committee, who

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qualifications, has served in the Medical Department of the Army or Navy; and the course must be of at least six months' duration, and comprehend not fewer than 60 lectures.

shall report their opinion to the College; and if the application is sustained, the individual so applying shall be enrolled in the usual manner.

7. All students, whose classes are thus registered, shall be entitled to receive from the Conservator a Certificate of their course of study, attested by his signature; but no class shall be included in this certificate unless actual attendance thereon have been previously attested by the Professor or Lecturer. No fee is payable for this certificate.

8. Such certificates shall be required from all candidates for Surgical diplomas, in regard to that part of their medical education which they may have received at Edinburgh. And similar certificates from any other school in which registration-books are kept, will be received, in regard to such portions of the course of study as the certificates may embrace. Candidates educated at schools where no registration-books are kept, must produce certificates from the respective Professors or Lecturers, both of their having entered to their several classes at an early period of the course, and also of their having attended these classes with due regularity. And in the case of Practical Anatomy, the certificate must express that the candidate has, during the course of his attendance, been actually engaged in the dissection of the human body, under the personal superintendence of the professor or teacher.

Chap. IV.—Exemptions.—Candidates who have commenced attendance on their Medical Classes, or at an Hospital containing at least eighty beds, or entered into Indentures of Apprenticeship to a regular Surgeon, previously to the following dates, are entitled to the exemptions which are here specified:—

Date.	Exemptions.
Previously to 29th Sept. 1838.	1. From more than four Winter Sessions' attendance on a School of Medicine.
Do.	2. From more than eighteen months of Hospital Attendance.
Do.	3. From more than six months of Practical Anatomy.
Do.	4. From the necessity of producing a Certificate of Age.
September 1833.	5. From Practical Pharmacy.
1st August 1831.	6. From Medical Jurisprudence.

Chap. V.—Examinations.—1. The days of Examination are the first and third Tuesdays of every month.

2. No candidate will be admitted to examination before the termination of his last year's course of study.

3. Applications for examination must be made to the President two days previously to the day of examination.

4. Every candidate, on applying to the President, will be required, 1st, To produce satisfactory evidence of his having attained the age of twenty-one; and, 2d, To present his certificates, and also a written statement (for which a printed form will be furnished by the Officer of the College), containing his Name, Age, and Country; a list of all the Classes, Hospitals, and Dispensaries, attended during each session of his study; and, if he has been an apprentice, the name of his master, the date of his indenture, and the length of time for which he was bound. This statement, properly filled up, must be attested by his signature.

5. If the candidate has been an apprentice to a fellow of the College for three years, he must also produce his discharged indenture. If for five years, he may apply at the end of four without losing any privilege, provided that he have a written permission from his master. The date of the indenture, and whether it is for the freedom or not, must be certified at the bottom of this written permission, by the Secretary.

6. On the production of these documents, the President will give the candidate a letter, authorizing the Examinators to take him on trial.

7. The Fees payable to the funds of the College must be lodged, before examination, in the hands of the Treasurer, who will certify this upon the President's letter, after inspecting and being satisfied with the certificates. The Fees will be returned to unsuccessful candidates, whose names will be concealed.

8. Unsuccessful candidates will be remitted to their studies for a period to be determined by the judgment of the Examinators, but not, in any case, less than three months.

9. The President, if he judge it proper, can order a meeting for examination on any day, at the request of a candidate; but, in that case, the candidate must pay Two Guineas in addition to the customary fees; and this money will not be returned to him, in the event of his being remitted.

10. Every candidate, at the commencement of his examination, will be required to translate into English some portion of a Latin Author, and if he be unable to do so, his examination cannot proceed.

11. Every candidate is expected to possess such a knowledge of drugs, and of the art of prescribing, as to be able to name particular substances that may be exhibited to him, and to write out formulæ for their administration.

Chap. VI.—Fees Payable by Candidates.—1. *For a Diploma*, the sum of L. 7, 5s.

[Apprentices of Fellows of the Royal College bound for the freedom, pay 25s.; their other Apprentices pay L. 2: 16: 6. Assistant-Surgeons in the Navy, having obtained Certificates from the College, pay L. 2: 11: 6. Surgeons in the Navy, having obtained Certificates from the College, pay 15s. 6d.]

2. *For the Certificate of Qualification to act as Assistant Surgeon in the Navy*, Candidates not having paid for any previous qualification, pay L. 4: 19: 6.

3. *For the Certificate of Qualification to act as Full Surgeon in the Navy*, Assistant-Surgeons who have already obtained Certificates from the College, pay L. 3: 18: 6, and those who have previously obtained the *diploma* of the College, pay L. 2: 17: 6

The sums stated above include all fees of every kind, and the officer is prohibited from receiving any.—By authority of the ROYAL COLLEGE.

(Signed) GEO. BALLINGALL,
President.

APPENDIX.—*Notice to Lecturers and Students.*

THE ROYAL COLLEGE OF SURGEONS OF EDINBURGH have enacted,—

I. That every Private Lecturer at this School of Medicine whose lectures are recognised by the College, shall ascertain, at least twenty times in a six months' course, and ten times in a three months' course, the actual attendance given by his pupils.

II. That he shall do this, by calling the name of each pupil at least once a-week, and entering in a regular roll-book the presence or absence of each individual,—the students, of course, being kept in ignorance of the particular day on which this is to be done.

III. That the said roll-book shall be kept according to a form prescribed by the College; shall be carefully preserved; and shall be at all times accessible to the President and Treasurer of the College, on both of whom it shall be incumbent to examine the roll-book of each class at least once during every session.

IV. That, in granting Certificates of attendance on their classes, the Private Lecturers shall use the annexed form, filling up the blanks with words at length, and on no account employing figures:

Edinburgh,
I hereby certify, that Mr attended my course of lecture
on commencing on and terminating o
that the roll of the class was called time
during the session; and that Mr was presen
on these occasions.
(Signed)

V. That, when a student's certificate of attendance bears that he ha been more than four times absent when the roll of his class was called, th President shall intimate this at the foot of that student's letter to the Examinators, specifying the branch, or branches, on which the defective attendance has been given, in order that he may be particularly examine upon them. But if a student's certificate of attendance bears that he ha been absent more than eight times when the roll of his class was called, h shall not be admitted to examination at all, until he has attended anothe course of lectures on every such department of study. These numbers t have reference to six months' courses, and one-half of the same numbe to regulate the procedure as to three months' courses.

VI. That when a student's absence has been occasioned by sickness, o any other unavoidable cause, such cause shall be stated in the certificat of attendance granted by the Lecturer; and the President and Council o the College shall decide whether the student shall, or shall not be require to attend another course of such lectures before presenting himself for examination.—By authority of the Royal College,

GEO. BALLINGALL, *President.*

SURGEONS' HALL, EDINBURGH,
21st October 1837.

Propositions relative to the Education and Privileges of Graduates in Medicine and Medical Practitioners, agreed on by the Medical and Surgica Professors in the University, the Royal College of Physicians, and th Royal College of Surgeons of Edinburgh. October 1838.—1. That th Legislature ought to fix a minimum course of *Education,* without certificates of having accomplished which, no one should be admissible to examination for a license to practise any of the branches of medicine.

Of General Practitioners.—2. That no person ought to obtain a licens to act *as a general medical practitioner,* who has not received a competen education in literature and science; studied in a recognised school of medicine or surgery; and undergone examination before a competent board o boards, on all the branches of medical education mentioned in the curriculum hereinafter specified.

3. That all public institutions, which have been usefully engaged in regulating the education, and ascertaining the qualifications of those intended for the medical profession (or such new boards as it may be found expedient to establish for the same purposes,) should be entitled to grant licenses conferring the right of acting as *general practitioners,* and of dispensing medicines in all parts of the British dominions: provided, *1st,* th course of education required by these institutions or boards, be equal o superior, in extent and duration, to that which shall be determined on a necessary for obtaining a license; and, *2d,* that the examining boards o these institutions be so constituted, as to afford a sufficient security tha the members of whom they are composed, possess the qualifications necessary to fit them for ascertaining, by examination, the proficiency of candidates.

4. That the time to be occupied in the minimum course of educati above-mentioned, at a University or recognised school, should not be le than twenty-seven months, in which should be included three winter-se

sions of six months' duration each; and that the age of the candidate for a diploma or license ought not to be less than *Twenty-one.*

5. That the classes required to be attended, and the hospital or dispensary attendance prescribed, in this minimum course of medical study, should be, as nearly as possible, of the same kind, and to the same amount, as are mentioned in the curriculum hereinafter specified.

6. That apprenticeship should not necessarily form a part of this education; but that those who have not been apprentices, should be required to bring proof of having acquired a knowledge of practical pharmacy in a laboratory or an apothecary's shop; and of having had opportunities of witnessing and assisting in the treatment of diseases, by acting, for a period of not less than six months, as pupils to practitioners in dispensaries, or in public hospitals receiving out-patients, or as pupils to regularly licensed private practitioners.

7. That evidence should likewise be required from the candidate for a license, of his possessing an adequate acquaintance with Latin, and of his having received instruction in the Elements of Mathematics, and in Natural Philosophy; and that it is highly desirable these branches should be studied previously to commencing the professional education.

8. That the examination for the license to practise, should be divided into at least two parts, to be held on different days; and that, *in Edinburgh,* these examinations may be advantageously conducted by a joint board of Fellows of the Royal Colleges of Physicians and Surgeons.

Of Surgeons.—9. That no person ought to obtain a license to act as a *surgeon,* who has not gone through a course of education at least equal in duration and extent to that laid down in the minimum schedule of education for the general practitioner, and undergone examination before a competent board.

10. That all persons having obtained a license to practise surgery, in conformity with the above conditions, should be entitled to act as general medical practitioners, and to dispense medicines in all parts of the British dominions, provided, in addition to an examination on anatomy, physiology, and surgery, they shall have been examined also on physic and pharmacy, and midwifery, by a competent board or boards.

Of Doctors in Medicine.—11. That the course of study and the examinations for the degree of M. D. of any University, ought to comprehend all the branches of knowledge stated above, and to imply a fuller and more complete education than is prescribed for the general practitioner.

12. That, on the other hand, the education for the degree of M. D. should not be raised so high above what is required of the general practitioner, as injuriously to limit the numbers of those persons who, in order to obtain the honour, may be willing to take a fuller course than is necessary for a simple license.

13. That, with these views, the education for the degree of M. D. should comprise a longer period of study than is required for the general practitioner, and *the age* at which the honour is conferred should be *above* that at which a license may be obtained.

14. That the superiority of the degree of M. D. granted by any University, should be further secured by the course of study required for it, embracing additional branches of science connected with medicine, and by enjoining repeated attendance on the more important departments.

15. That a certain portion of the study qualifying for the honour of M. D. should be prosecuted in some University which grants that degree, and that any such University ought to insist on attendance therein during a winter session, as preliminary and requisite to examination.

16. That all persons having obtained the degree of M. D. in conformity with the above conditions, should be entitled to act as general medical

practitioners, and to dispense medicines in all parts of the British dc nions.

Of Fellows of Colleges.—17. That no person should be held eligibl Fellowship in the Colleges of Physicians and Surgeons, 1*st*, Unless he s have obtained the degree of Doctor of Medicine, or the license of Gen Practitioner; 2*d*, Unless he shall have attained the age of twenty-thr and, 3*d*, Unless he shall have been engaged either in the study of Me cine, &c. in a recognized school for   years, or in the practic Physic or Surgery for   years, subsequently to the time of completing the course of study for the degree or license; and 4*th*, Unl he shall give such proofs as may be deemed requisite of an acquainta with the various branches of a liberal and scientific education.

Of Teachers.—18. That the lectures of Professors in Universities, F lows of Colleges, and persons hitherto recognized as teachers by the pu lic boards, ought equally to qualify for the license of General Practition and that those of the two latter description ought to be recognised in su extra academical education as may be allowed to qualify for the degree M. D.

19. That persons who are not Fellows of Colleges should be admit to the privilege of recognised teachers, on giving proofs of their possessi the same qualifications as are required from Fellows, and passing an e amination before a competent board.

20. That recognised teachers ought to give courses on the respecti subjects of their lectures, of such extent and duration as may be deem sufficient by the legislature to embrace the full consideration of these su jects.

21. That the lectures of no Professor or Lecturer who teaches witl the same year more than one of the branches required, ought to be recogn ed; but that, in reference to this regulation, Anatomy with Practical An tomy, and Chemistry with Practical Chemistry, may be considered as o branch respectively; while Clinical Medicine and Clinical Surgery m be taught, in addition to any of the other branches, by Professors, Phy cians and Surgeons, who are attached to recognised hospitals.

Of Chemists and Druggists.—22. That provision should be made in r gard to chemists and druggists for their being found sufficiently qualifi to compound, prepare, and dispense medicines; and that no persons oug to obtain licences to act as such who have not, 1*st*, attended at least o full course of lectures on each of the three following subjects, viz. Ch mistry, Botany, Materia Medica and Pharmacy, by recognized teache and, 2*d*, been employed for two years in compounding and preparing m dicines under a licensed general practitioner, or licensed chemist and dru gist.

23. That previously to obtaining such licences, the candidates sho undergo an examination on chemistry, botany, materia medica and ph macy, and as to their knowledge of the Latin language.

24. That those persons only who have gone through such education a examination, should be entitled to the name of licensed or approved ch mists and druggists, or to such other designation as may imply their qu fication; but that the license granted to them should infer no right to e ercise the duties of general practitioner.

Proposed Minimum Curriculum of Study for the License of General Practitioner.

Classes.	*Number of Courses.*	*Duration at least.*
Anatomy, . .	two courses	six months each.
Practical Anatomy, .		twelve months.
Chemistry, . .	one course	six months.
Practical Chemistry, .	do.	three months.
The number of pupils in each class being limited.		
Materia Medica and Pharmacy, .	do.	six months.
Institutions of Medicine or Physiology,	do.	do.
Practice of Medicine, . .	do.	do.
Clinical Medicine, .	do. or two courses	do. three months each.
During the period of attendance at the hospital in connection with which the lectures are delivered.		
Principles and Practice of Surgery,	two courses*	six months each.
Clinical Surgery, .	do. or two courses	six months. three months each.
During the period of attendance at the hospital in connection with which the lectures are delivered.		
Midwifery and Diseases of Women and Children, . .	one course	three months.
Medical Jurisprudence and Police,	do.	do.

Hospital Attendance.—Twenty-one months' attendance at a public general hospital, containing at least eighty beds; or eighteen months at such a public general hospital, and six months at a medical or surgical hospital or general dispensary.

N. B.—It is considered highly desirable that candidates for the licence of general practitioner should be required to produce evidence of having received practical instruction in midwifery, and also that they should avail themselves of the opportunities which they may possess of attending lectures on botany, natural history, comparative anatomy, and pathological or morbid anatomy, in addition to the courses of lectures absolutely required.

The foregoing suggestions are intended to form the basis of any general legislative measures that may be deemed expedient for the purpose of improving medical education, and conducting it upon some general principles.

The following tabular statement presents a view of the actual courses of study required by the different Boards specified, to qualify to undergoing examination for their respective licences to act as medical or surgical practitioner.

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* A course of lectures on military surgery, delivered by a professor of that branch in a university, or by a lecturer, who, in addition to the other required qualifications, has served in the medical department of the army or navy, might be admitted as an equivalent for the second course on the principles and practice of surgery.

Courses of Study for Medical Degrees a

	University of Edinburgh. Degree of M. D.	University of Glasgow. Degree of M. D.	Marischal College, Aberdeen. Degree of M. D.	University St Andrew Degree of M. D.
Preliminary Education, . .	Examination in Latin.	Examination in Latin.	Degree of A. M.	Degree of M. or Exa nation in L tin.
Period of Professional Study, .	4 Winter Sessions, each of 6 mon. duration.	4 Winter Sessions, each of 6 mon. duration.	4 Winter Sessions, each of 6 mon. duration.	4 Winter S sions. each o mon. durati
Anatomy, . .	1 course of 6 months, 110 Lectures.	1 Course of 6 months, 110 Lectures.	1 Course of 6 months, 110 Lectures.	1 Course of months, 11 Lectures.
Practical Anatomy,	6 months with Demonstrats.	....	6 months with Demonstrats.	6 months wi Demonstrat
Chemistry, . .	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mo 110 Lecture
Practical Chemistry,	....	....	60 Lessons.	60 Lessons
Institutions of Med. or Physiology, .	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mo 110 Lecture
Surgery, . .	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mo 110 Lecture
Military Surgery,	....	....	....	....
Clinical Surgery,	3 mon. 3 meetings per week.	....	3 mon. 2 Lect. per week.	....
Materia Medica and Pharmacy, .	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 m 110 Lecture
Practical Pharmacy,	6 mon. or Apprenticeship.	....	6 months.	6 mon. or A prenticeshi
Practice of Medicine, .	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 m 110 Lecture
Clinical Medicine,	6 mon. 3 meetings per week.	....	6 mon. 3 Lect. per week.	6 mon. 2 L per week.
Midwifery, & Diseases of Women and Children,	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	1 C. of 3 m 60 Lecture
Medical Jurisprudence, . .	1 C. of 3 mon. 60 Lectures.	....	....	....
General Pathology,	1 C. of 6 mon. 110 Lectures.	....	....	....
Botany, .	1 C. of 3 mon. 60 Lectures.	1 C. of 3 mon. 60 Lectures.	1 C. of 3 mon. 60 Lectures.	....
Natural History,	1 C. of 3 mon. 60 Lectures.	....	....	....
Hospital Attendance, . . .	18 months.	12 months.	24 months.	12 month
Apprenticeship, Dispensary Pract.	Apprentice. or 6 mon. Disp. Practice.	....	....	....
Age of Candidate for Degree or License,	21 years.	21 years.	21 years.	21 years

The number of Lectures stated is the Minimum. All the Courses of Lectures be taken in a University.

Licenses prescribed by the last Regulations of the

Royal College of Surgeons, Edinburgh. Diploma.	Royal College of Surgeons, London. License.	Royal College of Surgeons, Dublin. Letters Testimonial.	University of Glasgow. Degree of Magister Chirurg.	Faculty of Physicians and Surgeons, Glasgow. License.	Apothecaries' Company, London. License.
Exa. in Latin.					
Elem. of Math. 1 C. of Mechanical Philosop. 60 Lectures.	—	Examination in Latin and Greek.	Examination in Latin.	Examination in Latin.	Examination in Latin.
27 months; of these, 18 in Winter Sess.	4 yrs. profess. stud. includ. 27 ms. at a school.	4 yrs. profess. stud. includ. 18 ms. at a school.	18 Winter months at a School.	24 Wint. m. at Sl. or 18 Wint. and 9 Summer.	3 Winter and 2 Sum. Sessions at a School.
2 Courses of 6 months, each 110 Lectures.	2 Courses of 7 months, each 140 Lectures.	3 Courses of 6 months, 5 Lectures per week.	1 Course of 6 months, 110 Lectures.	2 Courses of 6 months, each 110 Lectures.	2 Courses of 7 months, each 140 Lectures.
12 months with Demonstrats.	12 months with 200 Demonstr.	18 months with Demonstrats.	—	6 months with Demonstrats.	6 months with 100 Demonstr.
1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon 70 Lectures.	2 C. of 6 mon. each 3 L. p. wk.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 100 Lectures.
60 Lessons.	—	Or 1 C., and 1 C., extent not stated.	—	60 Lessons.	Instruction, extent not stated.
1 C. of 6 mon. 110 Lectures.	—	—	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	—
2 C. of 6 mon. each 110 Lect.	2 C. of 6 mon. each 70 Lect.	3 C. of 6 mon. 3 L. per week.	1 C. of 6 mon. 110 Lectures.	2 C. of 6 mon. 110 Lectures.	—
Or 1 C., and 1 C. of 6 m. 60 Lectures.	—	—	—	—	—
6 mon. 3 meetings per week.	—	Instruction, extent not stated.	—	6 mon. 2 meetings per week.	—
1 C. of 6 mon 110 Lectures.	1 C. of 4 mon. 60 Lectures.	1 C. of 6 mon. 3 L. per week.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 100 Lectures.
6 mon. or Apprenticeship.	—	—	—	12 months.	5 yrs. Appren. to an Apothec.
1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 70 Lectures.	1 C. of 6 mon. 3 L. per week.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	2 C. of 6 mon. each 100 Lect.
6 mon. 3 meetings per week.	—	Instruction, extent not stated.	—	6 mon. 2 meetings per week.	Instruction, extent not stated.
1 C. of 3 mon. 60 Lectures	1 C. of 4 mon. 60 Lectures.	1 C. of 6 mon. 3 L. per week.	1 C. of 6 mon. 110 Lectures.	1 C. of 6 mon. 110 Lectures.	2 C. of 6 mon. each 60 Lect. Prac. Instruct.
1 C. of 3 mon. 60 Lectures.	1 Course of 25 Lectures.	1 C. of 3 mon. 3 L. per week.	—	1 C. of 6 mon. 110 Lectures.	1 Course of 60 Lectures.
—	—	—	—	—	—
—	—	—	—	1 C. of 3 mon. 60 Lectures.	1 C. of 3 mon. 50 Lectures.
—	—	—	—	—	—
21 months.	27 months.	36 months.	12 months.	18 months.	18 months.
—	—	—	—	—	Apprenticeship as above.
21 years.	21 years.	—	—	—	21 years.

scribed by the University of Edinburgh, and Marischal College, Aberdeen, are required t

At a numerous meeting of the Western Medical Society, held in Bandon, *Tuesday, the 21st of August* 1838, Daniel Donovan, of Union-Hall, Es M. D. in the chair,—The following resolutions, proposed by Dr Wood, Bandon, seconded by Dr Corbett of Innoshannon, were unanimously adop ed: Resolved, 1*st*, That we look upon the abandonment of the Medic Charities Bill, (Ireland,) with no small degree of alarm, as from th measure, we anticipated the most important and beneficial results to t Public, and the Medical Profession; and can only attribute its withdraw to that marked want of cordiality and mutual co-operation, which has t long disunited and influenced every branch of our Profession, in Irelan

Resolved, 2*d*, That we consider a monopoly, exercised by any bo of the medical profession, as highly injurious to general prosperity and su cess; as particularly favourable to the continuance of jealousy and di union; and as tending to depreciate its character in public estimation, a mar the prospects and advantages of its members individually.

Resolved, 3*d*, That we receive with much satisfaction, a propositio sanctioned as it is by a highly respectable and most influential body our profession in Ireland, (the Royal College of Surgeons,) for the ado tion of more general measures, and advocating Legislative enactments f securing an uniformity of education and privileges, among all the medic bodies throughout the empire; and we do not hesitate to declare th "this uniformity, if *freely* and *liberally* established, cannot fail to create union of interests, not only among the medical corporations, but among the medical men in the British dominions; and that such union must ha the effect of improving the condition of all."

Resolved, 4*th*, That we sincerely lament, that any misunderstandi should have produced the failure of the Medical Charities Bill (Ireland) and that we now pledge ourselves to support and advance, by every legi mate means, any proposed measures, that may tend to the promotion union and good feeling; the establishment of a judicious uniform standa of education, and a more equal extension of privileges; and to secure all classes of society those benefits, and that usefulness, which ought ev attend the due exercise of our profession, throughout the Empire.

Resolved, 5*th*, That we earnestly recommend our brethren, general throughout Ireland, to come forward, at the next Session of Parliamen and urge their claims upon the Legislature, by remonstrance and petitio in order to obtain the enactment of such measures, as may avert the serio and destructive consequences now pending, and afford that protection all our interests and privileges, which, as members of the community, have every right to look for and receive.—Signed by, and in behalf of t meeting, Daniel Donovan, M. D. Chairman; Samuel Wood, A. M. B. Secretary.

Bandon, August 22d, 1838.

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THE
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MEDICAL AND SURGICAL JOURNAL.
1. APRIL 1839.

PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*An Experimental Investigation into the Functions of the Eighth Pair of Nerves, or the Glosso-Pharyngeal, Pneumogastric, and Spinal Accessory.* By JOHN REID, M. D., Fellow of the Royal College of Physicians of Edinburgh, Lecturer on the Institutes of Medicine, Superintendent of the Pathological Department in the Royal Infirmary, President of the Anatomical Society, &c.*

IN a former Number of this Journal, (No. 134, January 1838, p. 109,) I detailed some experiments upon the three distinct nerves, which in this country are generally denominated the eighth pair, and also stated the conclusions which I believed may be legitimately deduced from them. I was obliged, for want of the requisite data, to defer the consideration of several important points connected with the functions of these nerves, and I have since that time been endeavouring to accumulate sufficient facts to supply some of those deficiencies. And as my remarks upon the functions of the pulmonary branches of the *vagus* were very cursory and incomplete, and as I did not even enter upon the consideration of the functions of the gastric branches of this nerve, I

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* A short epitome of this paper was read at the last meeting of the British Scientific Association.

intend, in the present communication, to confine myself chiefly t
their examination. Before proceeding, however, with what ma
be considered as forming the principal object of this communi
cation, I am anxious to make a few additions to and corrections
some of the statements contained in my former paper, which, in th
order of arrangement, precede the remarks which I have there mad
upon the pulmonary branches of the *vagus*.

Glosso-Pharyngeal Nerve.

At p. 123–4 of my former communication, I state, while mal
ing some remarks upon the reflex movements of the muscles of th
throat and lower part of the face, observed on irritating the trun
of the glosso-pharyngeal, that I had " endeavoured in several
the experiments, by gently pricking, pulling, and pinching t
nerve, to produce the usual muscular movements of deglutition
or " of those excited by disagreeable sensations in the fauces an
pharynx, but without effect." In an experiment which I perform
ed last summer, a distinct effort of deglutition was made ea
time the trunk of the glosso-pharyngeal was irritated; and th
under circumstances which, notwithstanding my numerous pr
vious failures, induce me to believe that this was not an accident
coincidence, but actually stood in the relation of cause and effe
I shall briefly relate the facts of the experiments as they were w
nessed by myself, so that every one may be able to judge how
I am justified in making this inference.

Exp. I.—The glosso-pharyngeal was exposed in a middl
sized terrier. The trunk of the nerve was pinched three tim
with the forceps, at intervals of two minutes, and each time a d
tinct movement of deglutition immediately followed. No su
movement was observed when the nerve was not irritated. T
animal was now deprived of volition by a dose of prussic acid, a
though the respiration went on for a short time, no effect f
lowed the pricking of the glosso-pharyngeal, and the irritati
even of the trunk of the *par vagum* appeared to have little eff
in exciting the respiratory movements.

This last fact ought not, we conceive, to be considered as at
riance with the first, for the prussic acid may, by its action up
the *medulla oblongata*, have rendered it considerably more obt
to impressions conveyed by the glosso-pharyngeal, and the trifli
effect which followed excitation of the *par vagum* may favour t
view. Besides, it must be remembered, in judging of the fu
tions of the glosso-pharyngeal as an exciter of deglutition, that i
pressions applied to the extremities of nerves generally act m
powerfully than when applied to their trunks.*

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* Volkmann has, from experiments upon the glosso-pharyngeal in the frog,

Pneumogastric Nerves.

I have again had ample opportunities of confirming the statement made in my former communication,—drawn from experiments upon dogs, rabbits, cats, and calves,—that the pinching, cutting, and even the stretching of the *vagi* nerves, when exposed in the neck, are, in by far the greater majority of cases, attended by indications of severe suffering. In opposition to the opinion expressed by Dr M. Hall and Mr Broughton, that the *nervus vagus* is not a nerve of sensation, I adduced the authority of Haller, Brunn, Dumas, and Dupuy. If additional evidence be thought necessary, I may also add to those the names of Molinelli, Mayo, Magendie, and Brachet. In the 1st and 5th experiments, upon the *vagus*, related by Molinelli,[*] it is expressly mentioned that the animals (dogs) gave indications of suffering, in tying these nerves with a ligature. Mr Mayo says, that " asses, cats, and dogs, almost invariably express great pain when this nerve, yet entire, is pinched with the forceps, and after its division equal suffering appears to result from pinching the part connected with the brain."[†] Magendie, in pointing out to his pupils an experiment where the nerve was stretched and cut without exciting pain, remarked, " In certain cases, on the contrary, the *nervus vagus* appears to possess the most exquisite sensibility; for it is scarcely touched without exciting immediately cries and convulsive motions."[‡] Brachet in one experiment irritated the upper end of the cut *vagus*, with the view of subjecting the animal to suffering, and with success.[§] I attempted to give an explanation of the source of fallacy which had misled Dr M. Hall and Mr Broughton, in their very limited number of experiments; but I am now convinced that there is another circumstance which is more likely to lead to such errors than the one I mentioned, and that is the very different degrees of sensibility possessed by different animals even of the same species. I have experimented on dogs which have endured the incisions necessary to expose the sheath of the carotid artery without any apparent uneasiness, and they remained quiescent though the *vagus* was violently stretched, pinched, and cut. Magendie, in explaining an experiment upon the *vagi* made before his pupils, adds, " The degree of sensibility in the pneumogastric nerve is variable; the division of this nerve is sometimes followed by acute pain; and sometimes, on the contrary, the animal seems scarce-

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rived at the conclusion which we have from experiments upon the dog,—that this is not a motor nerve.—Vide British and Foreign Medical Review, Vol. vii. p. 237, January 1839.

[*] De ligatis sectisque nervis octavi paris. In Comment. Bonon. Tom. iii. p. 280, 1755.

[†] Anatomical and Physiological Commentaries, No. ii. p. 15.

[‡] Leçons sur Les Phénomènes Physique de la Vie, Tom. i. p. 208, 1836.

[§] Recherches Experimentales sur les Functions du Système Nerveux Ganglionaire, Chap. i. Exper. 25.

ly conscious of the operation."* Such facts ought to make us sitate before arriving at negative conclusions upon the sensibil of nerves, and forcibly point out a serious error to which limi observations are liable.

In confirmation of an observation made by Dr M. Hall Mr Broughton, I formerly stated (p. 131,) that I had frequen repeated the experiment of compressing the *vagi* nerves in neck, and that in some cases powerful respiratory movements w produced. As some additional confirmation of this observati I may mention, that I remarked distinct respiratory moveme apparently excited in three animals by compressing the *vag* with the forceps, after they had been deprived of volition b dose of prussic acid. This experiment, however, fails much m frequently than it succeeds.

In a note at page 134, I stated, that it is perhaps not qu correct to say, that the pharyngeal branches of the *par vagum* f nish all the motor nervous filaments of the pharynx and soft late, and the more especially, as Palletta and Mayo had scribed, in the human species, a twig passing from the third bra of the fifth to the *circumflexus palati* muscle. With the vi of enabling us to decide whether or not the branches of the fi pair assist in moving the muscles of the soft palate, the followi experiment was performed.

Exp. II.—Three dogs were deprived of sensation and volit by small doses of prussic acid, and the skull-caps were sawn as expeditiously as possible, the root of the fifth pair exposed the cerebral side of the Casserian ganglion, and irritated b powerful galvanic battery. Before, however, the galvanic wi were applied to the nerve, the cheeks and the greater part of temporal and *masseter* muscles were rapidly divided, and the s palate and *isthmus* of the *fauces* fully exposed. On irritating nerve no movement of the muscles of the soft palate and *isthm* of the *fauces* could be detected, though the *elevator* muscu fibres which remained attached to the lower jaw, acted so pow fully, that it required a strong effort to prevent its closure. Af this had been repeated for a short time, the pharyngeal branch the *nervus vagus* was exposed and irritated in two of these a mals, both by the forceps and by galvanism, and distinct me ments of the soft palate followed. In these experiments, we m than once supposed that the parts about the *isthmus* of *fauces* moved on irritating the fifth; but were afterwards sa fied that this was mechanical, and dependent upon the convul movements of the muscles of the lower jaw; for when this b

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[*] Oper. cit. Tom. ii. p. 234, 1837.

was kept nearly fixed during the irritation of the nerve, the movements did not recur.

We believe, then, that in the function of deglutition the impressions are conveyed to the *medulla oblongata* along the branches of the glosso-pharyngeal; along the branches of the fifth pair distributed upon the *fauces;* and probably along those branches of the superior-laryngeal distributed upon the pharynx. The motive influence transmitted outwards from the *medulla oblongata* passes, we believe, along the pharyngeal branches of the *vagus;* along the branches of the hypoglossal, distributed to the muscles of the tongue, the thyro-hyoid, sterno-hyoid, and sterno-thyroid muscles; along the motor filaments of the recurrents ramifying upon the larynx; along some of the branches of the fifth supplying the elevator muscles of the lower jaw; along the branches of the *portio dura* ramifying upon the digastric and stylo-hyoid muscles and muscles of the lower part of the face; and probably along some of the branches of the cervical plexus, which unite themselves to the *descendens noni.** An examination of the various muscular actions engaged in deglutition will convince us that the muscles moved by the motor nerves we have enumerated, are employed in the performance of this function.

Laryngeal Branches of Vagus.—All the recent observations which we have had occasion to make on these nerves while performing other experiments, have only served to strengthen the conclusions drawn from our previous observations. At page 149, we stated, in making a few remarks upon the pathology of *laryngismus stridulus*, that "from the experiments we have detailed it is, however, apparent, that severe dyspnœa amounting to suffocation, may arise both from irritation and compression of the inferior laryngeal nerves, or of the trunks of the pneumogastrics." Without pretending to decide whether the disease we have referred to, can in many cases, or only in a few, be explained on Dr Ley's theory, I would merely remark, that in some experiments to which I shall again have occasion to refer, I have witnessed, after the division of the *vagi* in the middle of the neck and the consequent arrestment of the movements of the muscles of the arytenoid cartilages, sudden and violent attacks of dyspnœa, which generally went off in the course of a very few minutes, when they did not terminate in suffocation,—phenomena which have been considered by some as affording an insurmountable objection to the theory of Dr Ley, as it supposes the exciting cause of the disease to be permanent and not occasional. In these animals the respirations

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* We do not wish dogmatically to maintain, that the pharyngeal branches of the *vagus* may not include some sensitive filaments; but we believe, on the grounds stated in the former communication, that they are almost, if not entirely, motor.

were performed with ease after section of both *vagi*, as lon
as they remained at rest, or made moderate exertion; while,
soon as they began to struggle,—in other words, when the insp
ratory movements were performed with greater force,—sympton
of suffocation presented themselves, and these in some cas
shortly subsided, occasionally rather rapidly, on the anima
refraining from the violent exertion. We have also, in some
those animals, designedly brought on a paroxysm of dyspnœa, b
causing them to struggle.

I have lately had frequent occasion to experiment upon the i
ferior laryngeal nerves, and have observed that very few dogs gi
any indications of suffering when these nerves are irritated or cu
We are not, however, to conclude that they are entirely moto
for it must be remembered that many dogs remain perfectly qu
escent, if previously well secured, during the incisions necessa
to expose the nerve. Besides, the anatomical distribution of son
of the filaments of the nerve, and the fact, that animals do occ
sionally give indications of suffering on irritating it, are sufficie
to prove that it does contain some sensitive filaments.

Œsophageal Branches of Vagus.—Subsequent experimen
on rabbits have furnished me with results exactly similar to tho
I formerly detailed under this head. I have satisfied myse
however, that substances seem to pass pretty freely along t
œsophagus in most dogs after section of the *vagi*. Upon wh
this difference depends I have not yet been able to form any pr
bable opinion.

Cardiac Branches of Par Vagum.—Brachet relates an
periment, in which he states, that he tortured an animal (dog)
various ways, after having previously divided the *vagi*; and
though it manifested by its struggles and cries the pain which
suffered, yet the heart's action was not quickened, "le cœur
resté impassible, ses mouvemens n'ont pas varié." * This sta
ment is so much at variance with all we know of the physiolo
of the heart, that we might at once declare that some oversig
must have been committed. The movements of the heart
distinctly referrible to the same laws which regulate muscular c
tractility in other parts of the body, only somewhat modified
adapt it to the performance of its appropriate functions. Li
all the other muscles of the body, it is endowed with the p
perty of irritability, which enables it to contract upon the
plication of a stimulant; and whether we embrace the opini
that muscular irritability is dependent upon nervous influence,
adopt the much more probable doctrine, that it is a property
the muscular fibre itself, it is sufficiently proved,—and this is

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* Opus. cit. Chapitre premier, Exper. 25.

mitted by M. Brachet himself,—that the irritability of the heart is not derived from the *vagus*. The ordinary and habitual excitant to the irritability of the heart is the blood which is constantly flowing into its cavities. When the blood is forced on more rapidly towards the heart, as in exercise, its contractions become proportionally more frequent; and when the current moves on more slowly, as in a state of rest, its frequency becomes proportionally diminished. If the contractions of the heart were not dependent upon the blood, and their number not regulated by the quantity flowing into its cavities, very serious and inevitably fatal disturbances of the heart's action would soon take place. As statements such as those of M. Brachet are, however, more effectually met by facts than by arguments, I proceeded to put them to the test of experiment. These experiments were seven in number, and six of them were made in the following manner. The *vagi* and sympathetics,—and in some cases the recurrents also—were cut in the middle of the neck, and a portion removed. At a longer or shorter period after the operation the pulsations of the heart were reckoned when the animal was lying or standing on the ground, and after it had been caressed for some time to calm its fears. It was then lifted up on the table on which it had been previously tied and operated upon, and, after having been spoken to harshly, the pulsations were again reckoned. After being again caressed for some time the pulsations were counted a third time; and when replaced upon the ground they were reckoned a fourth time. The following results were obtained:—In the first dog, the pulsations of the heart were about 140 before the commencement of the experiment. The animal at this time was apparently somewhat alarmed. Four hours and-a-half after division of the nerves, the pulsations of the heart were about 170 when the animal was standing on the ground, and rose to 200 at least when placed upon the table. After it was replaced on the ground they had again fallen to about 170. After nineteen hours the pulsations were 160 on the ground; they rose again to about 200 when placed on the table,—again fell to about 160 when still on the table, and were not increased by being replaced on the ground. In the second dog the pulsations were 156 on the floor, and about 190 on the table; and in the third dog they rose 20 beats in the minute when placed on the table. In both of these two last experiments the pulsations of the heart soon subsided to their former frequency, and were not increased by replacing the animals on the ground. In the fourth dog the pulsations, twenty-four hours after division of the nerves, were 140 on the floor, and instantly rose to 180 on the table. After waiting until they had again fallen to their former frequency, they were not increased by replacing it on the ground.

In the sixth dog the pulsations were 140, the third day after t section of the nerves, when the animal was on the floor, and we raised to 160 by placing it on the table. In these experimen it was particularly observed, that the animals made no struggl in carrying them to and from the table, and, consequently, t increased excitation of the heart must have arisen from the ment emotion of terror. In the seventh dog this was conjoined wi violent struggles. The pulsations eight hours and-a-half aft the operation were 130; when placed on the table, and made struggle, the pulsations, as far as could be made out, were abo 220; when he had been subjected to pain, and had struggle more violently, they became so frequent that they could not accurately reckoned, but were at least 260 in the minute. large tube had been previously introduced into the *trachea* this last animal. These experiments are, we conceive, sufficie to prove that, after section of the *vagi*, the pulsations of the hea may not only be quickened by muscular exertion, but also by ment emotions. Though in all probability the *vagi* are the usual cha nels through which mental emotions affect the heart, yet it a pears from these experiments that this may also take place throu the medium of the ganglionic system of nerves.

Pulmonary Branches of the Vagus.—At p. 158 I gave t results of several experiments, from which, in opposition to the o servations of Magendie, Wilson Philip, and Swan, I conclud "that lesion of one of the pneumogastrics does not necessarily even generally induce disease of the lung of that side." Sin that time I have carefully examined the lungs of two dogs and cat, killed some time after a portion of one *vagus* had been r moved. One dog lived two months, the other nine days, a the cat three weeks. No morbid change could be detected in t lungs. I have now removed a portion of one *vagus* in sevente animals, which have been allowed to live a longer or shorter p riod,—from twenty-four hours to six months,—and in none of the could I detect any morbid change in the lungs which I could a tribute to the section of the nerve. In an experiment made Magendie before his pupils, the results were completely at varian with his former expressed opinions. The right lung of a dog, fro which a portion of the *vagus* of that side had been removed s months before, was on examination found to be perfectly healthy The circumstance of the lung remaining healthy after section the *vagus* of the same side might, I formerly supposed, be accoun ed for by the anastomoses between the pulmonary plexuses of t two sides. I now believe, however, that there is another cause operation which I shall have to discuss at some length when

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[*] Leçons sur les Phénomènes Physiques de la Vie, Tome i. p. 203-4.

come to examine the morbid changes in the lungs induced by lesion of both *vagi*.

Effects of Lesion of the par Vagum upon the Respiratory Muscular Movements.—It has now been fully ascertained by numerous experimenters, more especially by those who have investigated the functions of the *vagus* since the time of Legallois, that an animal will continue to breathe for a longer or shorter time, after this nerve has been cut on both sides of the neck, if care be taken to secure the free ingress and egress of air into the lungs. In my former communication I examined, under the section *Laryngeal Nerves*, the effects of lesion of the *vagi* in the middle of the neck upon the respiratory muscular movements of the *larynx*, and it will be here unnecessary to resume the discussion of that point. I there adduced various facts to show that when the *vagi* are injured above the origin of the inferior laryngeal or recurrents, the movements of all the muscles which enlarge or diminish the cavity of the *larynx* are arrested, and the superior aperture of the *larynx* can no longer be dilated during inspiration. If the *larynx* be large, and the animal refrain from any violent effort, an adequate quantity of air may still find its way through the diminished aperture of the *larynx*, and the respirations are at first performed with ease. If, on the other hand, the *larynx* be small, the quantity of air admitted through the interior of the *larynx* may be insufficient to carry on the respiratory process, and the animal may labour under dyspnœa from the moment the nerves are divided until its death. In young animals, in which the *larynx* is naturally small, and even not unfrequently in those full-grown, especially if the respiratory movements become forcible as in struggling,—or, in other words, when the capacity of the chest is suddenly and greatly enlarged,—the air rushes through the diminished aperture of the *larynx* in a narrower stream and with increased force, carrying the arytenoid cartilages mechanically inwards, producing the complete occlusion of the superior aperture of the *larynx*, and thus suffocating the animal.

As it becomes a matter of importance, in enabling us to arrive at some accurate conclusions upon the cause of the morbid changes induced in the lungs by lesion of the *vagi*, to ascertain the immediate effects of this operation upon the respiratory movements, I have of late attended particularly to this point: and have notes of thirty experiments in which the immediate effects of division of the *vagi* were watched upon dogs. In a considerable number of these, the recurrents were also divided. Seventeen of the experiments were performed chiefly for the purpose of investigating the morbid changes in the lungs. In the others I had different objects in view, and some of the animals were therefore allowed to live only a short time after the operation, but sufficiently long to ascertain its immediate effects upon the respiration. I need only state once for all, that in every one of these experiments the nerves were not simply divided, but a considerable portion was also removed. Many of the animals operated on were full-grown, so that no opening was made into the *trachea*. In others which were not full-grown a large tube was introduced into the *trachea* previous to the division of the nerves; and in a few in which (although they were full-grown) violent paroxysms of dyspnœa occurred after the nerves had been divided, an opening was immediately made into the *trachea* and a tube introduced. Such experiments are liable to several sources of fallacy, which must be carefully avoided, if we wish to arrive at accurate results. When an opening is made into the *trachea* under such circumstances, and no tube introduced, the incision in the skin and muscles must be very free, to secure a direct communication between the opening in the *trachea* and the external air, as the animal generally droops his head and the *trachea* is moveable. If a tube be introduced into the opening, care must be taken that it be sufficiently large, and not clogged up by blood or mucus, and that its shape be such as to prevent the orifice being obstructed in the bent position of the neck. Reflecting upon these inconveniences and sources of error, it occurred to us that by selecting aged animals, and after first ascertaining what effect the section of the recurrents had upon the supply of atmospheric air in the particular animal operated on, we might be enabled to judge more accurately of the extent of the influence of the *vagi* upon the other respiratory muscular movements. We believed that by selecting those animals in which the division of the recurrents produces no impediment to the respiration, the subsequent division of the *vagi* would produce no additional diminution of the supply of air through the *larynx*, and would consequently determine in a satisfactory manner the immediate effects of the lesion of the *vagi* upon the respiratory movements under circumstances where the egress and ingress of air to the lungs were quite free. Another difficulty, however, again presented itself, to which I have already alluded. In some of those animals the violent respiratory movements produced by the pain of dividing the *vagi*, or some powerful effort to liberate themselves before they were unbound, produced violent dyspnœa, which in some cases soon went off when the animal became more quiescent; but in others necessitated the opening of the *trachea* and the introduction of a tube. The idea of dividing the right *vagus* below the origin of the inferior laryngeals now occurred to us, but this being a severe and difficult operation was after one trial abandoned. It appeared then that the best plan to follow was to select full-grown animals; to avoid as much as possible any thing likely to produce the struggles of the animal, after, or during the division of the *vagi;* and if dyspnœa, threatening suffocation, should occur, quickly to introduce a pretty long and large bent tube into the *trachea*, and efficiently secure it. These details may appear uninteresting, and to many may seem unprofitable; but it is obvious that those who may wish to test the accuracy of the facts which I am about to mention, must repeat the experiments in a similar manner, unless it should appear that I have overlooked the agency of some extraneous circumstance which might interfere with the results. It is not so much the frequency of false observations that we have to complain of in medical science, as the errors which arise from making these observations under dissimilar circumstances, and from drawing general conclusions from insufficient data.

Of the thirty animals operated on, I find from my notes that fourteen, or rather less than one-half, had the *trachea* opened. Of these fourteen, the *trachea* was opened, and a tube introduced in nine, previous to the cutting of the *vagi*, as the age of the animal or the dread of the supervention of suffocation in some other subsequent experiment to which the section of the *vagi* was preparatory, made it prudent to proceed in this manner. In the remaining five, the *trachea* was opened subsequently to the commencement of the severe dyspnœa,—in four of these after section of the *vagi*, and in one after section of the recurrents, and before the *vagi* had been exposed. Of these thirty animals, I find that the respiration was easy immediately or soon after the division of the *vagi*, and this continued for a longer or shorter time in twenty-seven; while in the remaining three it is stated to have been difficult and approaching to dyspnœa. The *trachea* had been opened in these three animals, and in one of them, as no sufficient tracheotomy tube was at hand, none was introduced. In the other two, tubes were introduced; but I have every reason to believe that they answered the purpose very imperfectly. Besides, it ought also to be mentioned, that these three experiments were performed before I had matured my plans for providing for the passage of a sufficient supply of air through the *trachea*. I may also state here, that I could adduce several experiments, performed last year, which would seem to show that dyspnœa may occur immediately after section of the *vagi*, even where precautions have been used against it; and which at the time almost induced me to adopt the opinion, that the synchronous movements of the muscular fibres of the bronchial tubes were essentially necessary for healthy respiration. In reflecting upon the somewhat imperfect manner in which these experiments were performed, and finding that they are not confirmed by the twenty-seven experiments where all apparent sources of fallacy were avoided, I have arrived at the conclusion, that when a sufficient quantity of air traverses

the *trachea* after section of both *vagi* and recurrents, the respi
tions are at first performed with ease. Very important effects, h
ever, upon the respiratory movements manifest themselves insta
ly after section of the *vagi*. The frequency of the respirat
movements suffers a great diminution, and are at the same ti
performed more slowly, and generally even from the first in
somewhat heaving manner. I have notes, more or less comple
of the frequency of the respirations taken at different periods
ter section of the *vagi* in twenty dogs. We found it frequent
difficult, and in some cases impossible to reckon the number
respirations accurately in some of these animals previous to t
commencement of the experiment, owing to their restlessness, a
the gentle manner in which their respiratory muscular movemen
were performed; and this will explain some deficiencies in t
following tables. We were also sometimes not a little perplex
by the varying frequency in the number of the respiratory mov
ments in some of those animals, even within the space of a fe
minutes, and when no very apparent change had occurred
the circumstances in which they were placed. After the *va*
had been cut, we experienced much less difficulty in reckonin
the number of the respirations, as they were performed in a mo
slow and heaving manner. In the following table, I shall not a
range the experiments in the order in which they were performe
but classify them in the way which appears best adapted to sho
the results. The first table is for the purpose of pointing out t
difference between the number of respirations before and at va
ous periods after section of the *vagi*, and in the cases which I sh
afterwards mention of the recurrents also. I may state that t
observations were made in this manner. The respirations were rec
oned several times immediately before the operation was performe
—with the exception of Experiment XI. where the animal was r
peatedly examined for two days previous,—and then again imm
diately after the division of the *vagi*, and before the incisions we
stitched up. The T in the tables indicates that a tube was i
troduced into the *trachea*. I have marked at the end of som
of the experiments the time the animal lived after division of t
nerves, as we shall afterwards have occasion to refer to these.

TABLE I.

Expert.	Before operation.	Immediately after.			
3, T.	15	6-7			
4,	14-16	5-6			
			2 min.	5 min.	15 min.
5,	18-20	7-8	8	7-8	8-9
6,	16-17	6-7			

Expert.	Before operation.	Immediately after.													
			10 min.	15 do.	25 do.										
7,	16–18	8	12	7	7–8										
8, T.	24	8													
			10 m.	4 hours.	19 do.										
9, T.	24–28	14	12	14	dying.										
			2 hours.	8 do.	24 do.	32 do.	48 do.	4 days.	5 do.	6 do.	7 do.	8 do.			
10,	16	8	9	9	9	7	8	7	7	7	7	died.			
			2 hours.	24 do.	3 days.	4 do.	5 do.	6 do.	7 do.	8 do.					
11,	10–16	8	6–7	7	6	6	5	5	4–5	5	dying.				
			2 hours.												
12,	20–26	7–8	8–13	died two days after.											
			5 h.	6½ do.	11 do.	24 do.	2 d.	3 do.	4 do.	5 do.	6 do.	8 do.	9 do.	12 do.	
13,	16	8	7	7	7	8	8–12	7–8	7	7	7	7	7	7	killed.

TABLE II.

Expert.	Before Operation.	4½ hours After.	8 hours.	10½ h.	24 h.							
14, T.	16	7	7	7	died.							
		¼ hour.	19 h.	28 do.	48 do.	3 d.	5 do.	6 do.	7 do.	8 do.	9 do.	12 do.
15,	16	14	9	12–20	9	10	12	12	10	16	7	died.
		10 m.	2 h.	5 do.	9 do.	24 do.	32 do.	48 do.	52 do.			
16,	12	8	8	7	7	10	9	10	dying.			
		Soon after.	4 h.	8 do.	23 do.	28 do.	31 do.	34 do.				
17, T.	16	20	8	6	8	8	8	died.				
		8 h.	23 h.									
18, T.	16–18	6	8–10									

TABLE III.

	¾ hour.	4 do.	19 do.	21 do.	24 do.
19,	14	7	7	11	died.
	Soon after.	24 h.	48 do.	3½ d.	
20,	7	7	6	died.	
	7 h.	15 do.	23 do.	54 do.	
21,	12	8	12	died.	
	5 h.	24 do.	24 do.	69 do.	
22,	8	10–12	10–12	6–7	dying.

In examining the above tables it will be at once apparent in comparing the number of respirations immediately before and immediately after section of the *vagi*, that a very considerable diminution of the number of the respiratory muscular movements followed the division of these nerves, with the exception of Experiments XV. and XVII. It is stated in my notes, and the varying number of the respirations sufficiently indicate the fact, that the respiration was very easily excited in the animal the subject of Experiment XV. And as this was not remarked until the nerves had been divided, no safe conclusions can be drawn from such an experiment, especially as the respirations were not reckoned sufficiently often before the commencement of the experiment to ascertain their average frequency. The result of Experiment XVII. is so completely at variance with the others, that the circumstances connected with its performance require some explanation. This animal was one of those in which it was necessary to open the *trachea*, and introduce a tube after section of the *vagi*. It had suffered much from dyspnœa before the tube could be introduced, and the respirations were reckoned soon after the insertion of the tube, and before it had recovered from the effects of the dyspnœa. I may also state, that this was the first experiment which I made to ascertain the effects of the division of the *vagi* upon the frequency of the respiratory movements, otherwise I would have paid more attention to this circumstance, and would have again reckoned the respiration immediately after the animal had become calm. This experiment ought, perhaps, to have been rejected from the list, but I was anxious to give all the observations made on this point exactly as they are entered in my notes, so that every one may be able to judge of the soundness of the conclusions which we intend to deduce from them in a subsequent part of this paper. In judging of the effects of the division of the *vagi* upon the frequency of the respiratory movements, it is absolutely necessary to reckon them, for as these movements become more apparent, prolonged, and heaving, we may be readily deceived. From neglecting this, I fell, in my earlier experiments upon the *vagus*, into the error of supposing that the respiratory movements were increased in frequency, especially in rabbits. It is in this manner that we can account for the statement of Dr M. Hall, that after division of the *vagi* "the acts of respiration become much more frequent."* At a shorter or longer period after section of the *vagi*, the respirations become more heaving and prolonged, while the expirations continue to be comparatively short and rapid, and frequently attended by a sound caused by the sudden expulsion of the air.

In some cases the number of respirations was afterwards still farther reduced; while in others it varied at different periods; and in a few it became more frequent shortly before death. At a longer interval after section of the nerves, the inspirations generally become still more heaving and prolonged; the blood is less perfectly arterialized in the lungs, and the arteries circulate blood gradually approaching to the venous character; the animal becomes more dull and stupid, the evolution of animal heat is diminished, and it dies asphyxiated.

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* Memoirs on the Nervous System, p. 85, 1837.

We believe that it is sufficiently evident from these experiments, that the *vagi* are important nerves in transmitting those impressions to the *medulla oblongata* which excite the respiratory muscular movements; and they may appear to favour the opinion of Dr M. Hall, that the continuance of the respiration, after division of the *vagi*, is a voluntary and not an excited act.* In my former communication, (p. 161,) I stated that I had observed respiratory movements continue in animals after they had been deprived of all volition by a small dose of prussic acid. I have since made similar experiments upon animals deprived of volition by alcohol, and by blowing air along the carotids towards the brain. I am also satisfied that, in such experiments, the inferior laryngeal nerves may be also divided without affecting the results. As, however, it has been stated by Dr M. Hall, on the authority of Cruveilhier, (Lancet, February 17, 1838, p. 733,) that after the function of the cerebrum is destroyed, or, in other words, after the animal has been deprived of volition, the respiration ceases instantly on dividing the pneumogastrics near their origin, I thought it necessary to repeat this experiment.

Exp. XXIII.—The *vagi* were first exposed in the upper part of the neck in a puppy six days old. A considerable portion of the *cranium* was removed on both sides of the superior longitudinal sinus, and the hemispheres of the brain were completely destroyed down to the *corpora quadrigemina.* The *vagi* were then cut, and the *trachea* opened, without arresting the respiratory movements. The animal was evidently much exhausted from loss of blood, and the respirations were performed at long intervals even before the *vagi* were divided. After the section of the *vagi* the respiration went on for several minutes, during which the *cerebellum* was also broken up by the forceps.

Exp. XXIV.—The *vagi* were exposed and the hemispheres of the brain removed, as in the preceding experiment. The respirations continued after the division of the *vagi.* A few minutes after division of the nerves, the respirations were reckoned and found to be 4 in the minute. The recurrents were then divided, and the *cerebellum* broken up as cautiously as possible by the forceps. Seven minutes after the respirations had been first reckoned, they were between 3 and 4 in the minute; at the thirteenth minute they were still between 3 and 4. The spinal column was completely severed between the second and third cervical vertebræ fifteen minutes after the respiration had been first reckoned, and

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* Oper. cit. p. 87.

the respiratory movements still continued at the rate of 1 in
minute. At the twentieth minute the head was entirely rem
from the body, and some cold water was dashed over the face
it had lain quiescent for nearly two minutes, and the muscl
the face moved as in inspiration. In about two minutes mo
similar movement followed without the artificial application o
external excitant. Between the seventh and thirteenth mi
after the respiration had been first reckoned, the animal repeat
sucked the finger introduced into the mouth, and at the fifte
minute it still did this though feebly.

These two experiments being positive ones are sufficient, i
pendent of others which we could adduce, to prove that the r
ratory movements are not immediately arrested after remova
the *cerebrum* and *cerebellum* and division of the *vagi*.
have seen that division of the *vagi* in an animal in which the
cephalon was uninjured was immediately followed by a dimin
ed frequency of respiration; and it becomes an interesting q
tion to ascertain what is the effect of the lesion of those nerve
an animal deprived of volition by the previous removal of
brain, as such experiments will determine the influence of
vagi as exciters of respiration in a more precise manner than t
performed upon animals in which the respiratory movements
be readily modified by volition.

Exp. XXV.—The pneumogastrics were exposed and surrou
ed by a loose ligature in a kitten one day old. The hemisph
of the brain were removed, and attempts were made to break d
the *cerebellum* without removing the osseous *tentorium*.
fore the commencement of the operation the respirations w
nearly 100 in the minute; after removing the hemispheres of
brain they were about 40. On cutting the *vagi* they insta
fell to between 3 and 4 in the minute, and the animal contin
to breathe in this manner for an hour, when it was left. Du
this time it sucked the finger introduced into the mouth.

Another experiment of the same kind was performed on a
ten of the same age, whose respirations were also about 100 in
minute. This animal appeared to be more exhausted from hem
rhage, and only breathed twelve times in a minute after the rem
al of the hemispheres. After the section of the *vagi* the respirati
were only between 2 and 3 in the minute; and it continued
breathe in this manner for a quarter of an hour, when it was
On examining these two animals after death, it was ascertai
that the whole of the *cerebrum* down to the *corpora quadri*
mina had been removed, and that the *cerebellum* was only
tially injured. I find it stated in my notes of these two exp
ments, that, if the animals had been left immediately after sect
of the *vagi*, we might have gone away with the impression

they had ceased to breathe, as the respiration appeared quite arrested for about a minute.

Exp. XXVI.—The *vagi* and recurrents were exposed, the hemispheres of the brain removed, and the *cerebellum* injured, in a kitten five days old, as in the previous experiments. The respirations before the commencement of the experiment were 120; after removal of the hemispheres of the brain 40; after section of the *vagi* and recurrents they instantly fell to 4 in the minute; two minutes after this they were between 3 and 4: five minutes after they were 4; and seven minutes after they were still 4. It was now pithed at the lower part of the neck, between the sixth and seventh cervical vertebræ, and the respirations fell to one in the minute. It continued to breathe in this manner for seventeen minutes, when it was left.

A similar experiment was performed on another kitten of the same age. The respirations were few and irregular after the removal of the cerebral hemispheres. The *vagi* were then cut, and it continued to breathe about once in the minute for half an hour, when it was left. On dissecting these two animals after death, it was found that a considerable part of the *cerebellum* had escaped injury in the former; while in the latter, nothing except the *medulla oblongata* had been left within the cranium.

While these experiments illustrate the great importance of the pneumogastrics as exciters of respiration, they also prove that the impressions made on the filaments of the pneumogastrics distributed in the air-cells of the lungs are not the sole excitants of respiration; and that there are other nerves which can transmit impressions to the *medulla oblongata* capable of exciting the involuntary respiratory movements.

I formerly suggested (p. 162) that the continuance of the respiration after section of the *vagi* might depend upon the transmission of impressions along the sympathetic branches distributed in the lungs, backwards to the spinal chord. Several facts related in the above experiments upon the effects of the division of the *vagi* after removal of the brain seem to prove that all the impressions conveyed to the *medulla oblongata* capable of exciting the involuntary muscular movements under such circumstances, are not transmitted through the sympathetic system. I need only refer to the gasping of the animal after decapitation—a fact formerly observed by Legallois,—and to the continuance of the respiration after the pithing of an animal in the cervical region, from which the brain and *cerebellum* have previously been removed, and the *vagi* divided high in the neck. I may also state, that I have seen the respiratory muscles twice called into simultaneous movement after all the thoracic viscera had been rapidly removed in an animal deprived of sensation and volition by a dose of prussic acid. The facility with which the respiratory movements are excited by impressions made upon the sensiferous filaments of the fifth pair,—well illustrated by the effects of dashing cold water on the face,—will at once suggest this nerve as a likely channel through which these impressions may be conveyed. The effects of certain impressions upon the surface of the skin generally, and the details of two very interesting and important cases of the resuscitation of new-born children by the contact of the cool atmosphere with the surface of the body, given by Dr M. Hall, on the authority of Dr Henning * and by Dr Wagner, † point out the filaments of the spinal nerves distributed on the skin as also probably engaged in transmitting those impressions which excite respiratory movements. It would be difficult to devise experiments likely to afford such decisive results as would enable us to ascertain the relative share which these different nerves have in carrying on the respiration after section of the *vagi*. The results of experiments XXIII. and XXV., where the respiratory movements suffered another very apparent diminution on pithing the animal in the cervical region, would seem to show that the nerves distributed about the face and upper part of the neck do not certainly furnish the only channels through which the impressions which excite respiration are conveyed to the *medulla oblongata* after division of the *vagi*. We must, however, be cautious in drawing conclusions from negative experiments so few in number. With the view of throwing further light upon this question, the following experiment, which is similar to one detailed by Mr Cruickshank, ‡ was performed.

Exp. XXVII.—The *vagi* and sympathetics were divided low in the neck in a powerful bull-bitch at 8 o'clock A. M. A large tube was introduced into the trachea before the division of the *vagi*. The respirations before the operation were from 16 to 18 in a minute. After the operation it breathed freely through the tube and walked about. At 4 P. M. the respirations were 6 in the minute, when it was lying at rest. Next morning at 7 o'clock the respirations were reckoned repeatedly, and were found to be from 6 to 7 when lying, and from 8 to 10 when standing. It was now pithed at the lower part of the neck, and it continued to breathe by the diaphragm alone, and appeared uneasy during the short time it was allowed to live. The respirations were reckoned between two or three minutes after the spinal chord had been cut across, and were 6 in the minute. For twenty minutes after this they were reckoned at short intervals, and were still 6 in the minute. Both femoral arteries were now opened, and the blood, though of arterial hue, was darker than usual, and flowed out with diminished impetus. No satisfactory conclusions can be drawn from this experiment, as the frequency of the respiration was probably influenced by volition and other causes. As far as a single experiment of this kind can go, however, it appears to show that the frequency of the respiratory movements was somewhat diminished by the pithing of the animal, for I am convinced that the respirations would have been more frequent had the animal been as much excited before it was pithed as it appeared to be afterwards. In Mr Cruickshank's experiment the animal died sixteen hours after the operation. The number of respirations varied from 5 to 15 in a minute. Their frequency before the operation is not stated.

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* Memoirs on the Nervous System, p. 88.

† British and Foreign Medical Review, Vol. v. p. 582.

‡ Medical Facts and Observations, Vol. vii. Exper. vii. p. 147.

The diminution of the frequency of the respiratory movements consequent upon division of the *vagi* has been observed by other experimenters. A. G. F. Emmert concluded, but apparently more upon theoretical grounds, than from any direct observations made in the two experiments which he had at that time performed on rabbits, that after lesion of the *vagi* the respirations became less frequent and prolonged. [*] Mayer reckoned the number of respirations, both before and at various periods after section of the nerves, in five experiments, and gives the results in the following table.

	Before operation.	After operation.
Ass,	17	8
Dog,	48	8
1st rabbit,	80	40
2d do.	100	48
3d do.	80	28[†]

The late Mr Broughton mentions, that in a horse in which the *vagi* were divided, "the respirations became slow, 12 in a minute."[‡] In another horse, "the respirations fell to five in the minute."[§] At what period after the operation this diminution in the frequency of the respirations was observed, and what was the number of the respirations previous to the experiment, we are not informed. Sir Astley Cooper has given the results of two experiments upon rabbits, which well illustrate the effects of section of the *vagi* upon the frequency of the respiratory movements. In one rabbit the respirations were 132 in the minute before the commencement of the experiment. One hour after division of the nerves they were 48; and after eleven hours and a-half they were only 30. In the second experiment the respirations were 135 before the division of the nerves. One hour after, they were 48; six hours after, they were 36; and after sixteen hours they were 28.* The experiments we have related prove that this diminished frequency of the respiration instantly follows the division of the nerves, and cannot depend upon any blunting of the sensation from the circulation of dark blood in the arteries of the brain; though this no doubt may, after a while, probably assist in diminishing them still farther. We shall have occasion to examine the probable influence of the diminished frequency of the respirations in producing the morbid changes which are observed in the lungs, when we come to discuss that part of our inquiry.

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[*] Archiv. für Physiologie, von Reil und Autenrieth. Neunter Band, 1809. S. 417.

[†] Tiedemann's Zeitschrift für Physiologie. Zweiter Band, 1826, S. 77.

[‡] Quarterly Journal of Literature, Science, &c. Vol. x. p 305.

[§] Oper. cit. p. 307.

Are the Pulmonary Branches of the *Vagus* both Motor and Sensitive Nerves? We have made various attempts to obtain some satisfactory evidence that the muscular fibres of the bronchial tubes are moved through the influence of the *vagi*, but we must confess that these as yet have all failed. I have frequently performed the following experiment upon animals immediately after death. An opening was made into the *trachea*, into which the broad end of a bent tube of the form of a blowpipe was inserted and there secured, while the narrow end was introduced below the surface of water in a shallow vessel. The nerves were then immediately exposed, and irritated both by the scalpel and by galvanism; and we anticipated, that, if the muscular fibres of the *bronchi* were thrown into contraction, a part of the air would be forced along the tube, and rise through the water in the form of bubbles. No such appearance, however, presented itself, though the muscular fibres of the œsophagus moved at each application of the excitant. We do not, however, consider this a very delicate method of testing this point, from some circumstances which we observed in performing these experiments. In one or two cases in which I believed I heard the natural respiratory murmur after section of the *vagi*, I could detect no change in the respiratory sound when the lower end of the cut nerve was pinched by the forceps during inspiration. The restlessness and tremblings of the animal, generally, however, interfere with the accuracy of such observations. We certainly do not consider these negative experiments as by any means sufficient to entitle us to conclude that some of the pulmonary branches of the *vagus* are not motor nerves—the more especially as the well-known symptoms of what is called spasmodic asthma, and certain phenomena observed by Laennec on applying the stethoscope over the chest,† render it exceedingly probable that muscular movements occur in the bronchial tubes; and if such movements be influenced by nerves, it is much more likely that these should belong to the *vagus*, than to the sympathetic system. M. Brachet has related some experiments, which, if correct, prove in a most satisfactory manner that part of the pulmonary branches of the *vagus* are motor.[*] We have not repeated those experiments; but, from an examination of the details given by M. Brachet, we cannot help suspecting that some error must have been committed in their performance, which had been overlooked.

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* Guy's Hospital Reports, No. 3, September 1836, p. 409.

† Treatise on the Diseases of the Chest, &c. p. 418, 1829, Forbes' Translation.

From the distribution of the *vagus* upon the bronchial tubes we might conclude *à priori*, that it must be concerned in conveying those impressions to the central organs of the nervous system which excite sensations. It does not, however, necessarily follow, that division of the *vagi* should annihilate all the sensations referred to the lungs. I formerly stated, that I was convinced, from frequently repeated experiments, that section of the *vagus* on both sides neither arrests the transmission of those impressions to the *medulla oblongata* which excite the involuntary respiratory movements; nor does it annihilate the sense of anxiety arising from the deficiency of fresh air in the lungs. I have since that time had frequent opportunities of verifying this statement, and I am confident that animals will evince great uneasiness—to judge from the struggles of the animal—after section of these nerves, when the access of air to the lungs is prevented. It is possible, however, that this *besoin de respirer* may be diminished by section of the *vagi*, though it certainly is not annihilated. In order to perform such experiments properly, it is necessary that the access of air to the lungs be fully and suddenly prevented. Such experiments as those performed by Brachet[†] and Mr Grainger,[‡] where the animal was placed under a bell-glass in a limited quantity of air, are liable to an obvious source of fallacy. When carbonic acid is accumulated slowly in the air inspired even in a healthy individual, a torpor gradually creeps over the nervous system, and the sense of anxiety at the chest is never very urgent, and frequently not sufficient to rouse a person from his sleep. The melancholy fate of many an unfortunate individual could be easily adduced in confirmation of this. I have seen rabbits confined under a bell-glass until their breathing became quick and heaving, without their evincing any marked uneasiness, and without making efforts to escape. The other method of experimenting followed by Brachet, viz. of cutting the *vagi* in a puppy three days old, and then placing the nose under water,[§] is equally liable to error. From the various experiments we have made, we are led to believe that section of the *vagi* in an animal so

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[*] Opus cit. p. 299, Experiment 125–6.
[†] Ibid. Experiment 37, p. 134.
[‡] On the Spinal Chord.
[§] Opus cit. Experiment 34.

young is followed by the instant occlusion of the superior ap
ture of the *larynx*, so that the struggles and uneasiness arisi
from the occlusion of the *larynx* may have ceased before the no
of the animal was placed under water. At all events, we fou
on repeating this experiment on kittens, that plunging the nose
water was a very useless proceeding, for violent struggles and e
dent uneasiness instantly followed the division of the *vagi*.

In our last communication, (p. 162,) we alluded to the expe
ments made by Brachet, from which he concludes that all the sens
tions occasioned by foreign bodies, &c. in the air-passages are d
pendent upon the integrity of the pneumogastrics. We stated th
we had never yet been able to induce the severe paroxysms
coughing described by Brachet, by any mode of irritating the i
ner surface of the *trachea* which we have adopted. To show th
others have also sometimes observed great insensibility of t
mucous membrane of the *trachea*, even when the *vagi* were e
tire and uninjured, I have only to refer to the works of Haller.
He there relates several experiments upon different species of qua
rupeds,—two cats, a she-goat, a rabbit, a lamb, a he-goat, and
sheep—in which the *trachea* was opened, and various irritati
substances, such as oil of vitriol, butter of antimony, and fum
of sulphur, were introduced into the air-passages, without exciti
cough. Some of the animals gave indications of suffering, a
breathed forcibly. The sheep was supposed to be phthisic
Every practical surgeon is aware of the little uneasiness produc
by the introduction of a canula into an opening in the *trache*
The violent paroxysms of coughing consequent upon the introdu
tion of foreign bodies into the *trachea* from the mouth seem pri
cipally dependent upon their passage through the *larynx*. I ha
suggested that Brachet in some of his experiments may have ov
looked a source of fallacy which would very seriously interfere wi
the results,—and that is, the facility with which some of the ir
tants used might reach the interior of the *larynx* ; although it
to be remembered that, in the experiments which he details to
lustrate the annihilation of the sensibility of the inner surface
the *trachea* and bronchial tubes after division of the *vagi*, the nerv
were cut in the middle of the neck. It is nevertheless quite possib
that this may affect the results in some experiments more than
others, and it is evident that, if the head be depressed when
quantity of fluid is thrown into the *trachea*, or if the fumes fro
a muriatic acid bottle be used as an irritant (as by Brachet in so
of his experiments,) the chances are, that a part of these will rea
the interior of the *larynx* and excite violent efforts to cough.†

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* Opera Minora, Tom. i. p. 402, Laus. 1762 ; or Sur la Nature Sensible et
ritable, Tome i. p. 394, Laus. 1756.

† Müller refers to the experiments of Krimer, as corroboratory of those by B

the last communication we illustrated at some length (p. 144,) the great difference in the acuteness of sensibility between the mucous surface of the *larynx*, and that portion of the air-passage placed below it; and the same thing was remarked by Mr Key in an operation on an individual of the human species for the extraction of a foreign body from the *trachea.*[*] I have performed several experiments with the view of satisfying myself regarding the effects of the division of the *vagi* upon the sensibility of the mucous membrane; and though I have seen good reason to believe that it is much blunted, I am by no means satisfied that it is entirely annihilated. I must also state that it is very difficult to procure a decisive experiment on this point. The animal frequently becomes restless and uneasy after section of the nerves, if care be not taken to secure a sufficient supply of air to the lungs; and it may make forcible expirations not unlike a cough when the mode of irritating the inner surface of the *trachea* is such as to interfere materially with the passage of the air along it. This, however, in all probability, may arise from the sense of anxiety occasioned by the impediment to the respiration. Another source of error arises from the great facility with which the air-passages tolerate stimulants, which at first excite great uneasiness, as was well illustrated in the practice followed by Dessault.[†] I shall now detail a few of the experiments which I made, to illustrate these sources of fallacy I have mentioned, and also show that the sensibility of the mucous membrane of the *trachea* and *bronchi* is at least blunted after division of the *vagi*.

Exp. XXVIII.—The pneumogastrics were exposed without disturbing them in a large terrier dog. A small opening was then made into the *trachea*, and a quantity of water was thrown down towards the lungs (the head being kept erect) without exciting cough. A small quantity of alcohol was then injected downwards, and this was followed by efforts to cough. After these had ceased, it was thought advisable to witness their repetition. A fresh quantity of alcohol was therefore injected downwards, but the animal remained perfectly quiescent. This was again repeated with the same results. It was now apparent that it was useless to proceed farther, so that, instead of dividing the *vagi*, we injected half an ounce of air (by measure) along the carotid artery towards the brain. This was followed by strong convulsions of the limbs and trunk, succeeded by coma, which continued for at least four hours,—the time it was

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chet on this subject. (Baly's Translation, Vol. i. p. 353.) As I have not yet been able to procure the treatise by Krimer in which these experiments are detailed, I am ignorant of the manner in which they were conducted.

[*] Lancet, 1828–9.

[†] Oeuvres Chirurgicales par Bichat, Tome ii. p. 266, 1801.

last seen alive. It occasionally howled and moved its limbs rapidly.*

Exp. XXIX.—The *vagi* were exposed in the middle of the neck, but not disturbed, in an ordinary sized terrier. A small opening was then made into the *trachea*, and some cold water injected downwards, the head being kept erect. This was followed by slight efforts at coughing. The nerves were then divided, and the animal became restless and uneasy. Both water and alcohol were injected downwards after division of the *vagi*, without exciting cough as long as the head was kept erect, while it occurred when the head was placed in a depending position.

The alcohol injected was at first in small quantites at a time, and after short intervals. The whole quantity injected was two drachms, and this produced some symptoms of narcotism.

Exp. XXX.—The *vagi* were exposed in the middle of the neck, but not disturbed, in a very lean and large mongrel dog, lately recovered from the distemper. A very small opening was made into the *trachea*, and a quantity of cold water thrown downwards without inducing cough. A small quantity of alcohol was then injected, and three or four distinct coughs followed. The *vagi* were then divided, and this was followed by no impediment to the respiration. Alcohol to the extent of half an ounce in all was thrown in divided quantities down the *trachea*, without exciting any efforts to cough. The animal was at last evidently affected by the alcohol, for when let loose it staggered in walking.

Exp. XXXI.—The *vagi* were exposed without being disturbed in a young middle-sized mongrel. Water injected into a small opening in the *trachea* excited pretty urgent cough, and it appeared uneasy and coughed frequently when alcohol was injected. Section of the *vagi* was followed by struggles and symptoms of suffocation. It recovered from this apparently to a considerable extent; and some alcohol was then injected into the *trachea* without exciting uneasiness or cough. A drop of muriatic acid was also placed in the *trachea* without exciting uneasiness. Some alcohol was now injected towards the *larynx*, and this was followed by uneasiness and cough. The animal was now killed by a dose of prussic acid. In none of these three last experiments was any cough excited by irritation of the inner surface of the *trachea* and *bronchi* after division of the *vagi*, but I could adduce two experiments where a large tube was first introduced into the *trachea* before the division of the *vagi*, in which uneasiness and forcible expirations, which I could not distinguish from an effort to cough, were excited, when irritating substances were injected into the *trachea*. The details of Experiment XXVIII. are very instructive. If we had proceeded to divide the *vagi* after the first injection of the alcohol without repeating it, we might have attributed the different results which followed, entirely to the section of the nerves. We cannot assert that the same thing may not have occurred in one or more of the three other experiments. It is worthy of remark, that in four dogs which lived beyond the fourth day after section of the *vagi*—in three of which the recurrents were also divided—frequent cough, or at least forcible expirations, which I could not distinguish from a cough, were observed. The animal the subject of Experiment XIV. coughed so incessantly during the three last days of its life, that I could not reckon the respirations until after many trials, and even then imperfectly. Whether this was dependent upon any irritation of the upper part of the cut nerves, or upon any irritation of the mucous surface of the lungs, or upon any other cause, I cannot pretend to determine. I may, however, mention, that the examination of the cut ends of the nerves after the death of these animals did not disclose any marks of high inflammation. In an experiment by Valsalva, where the *vagi* were cut in a young dog which lived to the eighteenth day, the animal is described as coughing on the third and some of the subsequent days.*

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* I have repeated this experiment of blowing air along one of the carotids or vertebrals towards the brain, in animals which had not been previously subjected to the influence of any narcotic, and always found that it produced convulsions and coma, lasting for some time before death. The quantity of air injected was from half an ounce to an ounce, and even more. The mode of death in such cases is very different from what I have observed when air is injected *rapidly* and in *considerable* quantity into a vein leading directly to the heart, for it then kills by mechanically arresting the movements of the right side of the heart.

Morbid Changes in the Lungs.—I have removed a portion of both *vagi* in seventeen dogs, for the purpose of examining the morbid changes in the lungs consequent upon section of these nerves. In ten of these the recurrents were also divided. The time which twelve of them lived after division of the nerves is given in the tables illustrating the diminution of the frequency of the respirations after this operation. I may also add here, the period of time which the other five lived. In Experiment XXXII. the animal lived thirty-one hours; in Experiment XXXIII. it lived thirty hours and a-half; in Experiment XXXIV. it was killed after ninety-six hours; in Experiment XXXV. it lived fifty-five hours; and in Experiment XXXVI. it lived twenty-one hours. In all of these last experiments except the XXXVth, a tube was introduced into the *trachea*. We find on examining the period of time which these seventeen animals lived, that four died before the completion of twenty-four hours, and in three of these a tube had been introduced into the *trachea;* three died between the twenty-fourth and forty-eighth hour—all of which had tubes introduced into the *trachea;* four died between the forty-eighth and seventy-second hour, none of which had tubes introduced into the *trachea;* one died between the seventy-second and eighty-sixth hour; in this animal no opening was made into the *trachea;* one was killed after four days and eight hours, and, though an aged dog, had an opening made into the *trachea;* two died after the completion of the eighth day; one died on the twelfth day; and one was killed after the completion of the twelfth day. In the last four experiments the animals breathed freely without opening the *trachea.* To these seventeen experiments I could also add several others, where the animals were killed at longer or shorter periods after division of the nerves, and to some of which I shall have to refer. The young dogs subjected to these experiments generally died earlier than the more aged, as may be inferred from the circumstance, that the greater number of those which lived longest did not require the introduction of a tube into the *trachea.* The lungs were in a state unfit for the healthy performance of their functions in fifteen out of the seventeen animals experimented upon. One of the two animals in which the lungs were found healthy (Experiment X.) died after the completion of the eighth day, apparently from inanition; the other (Experiment XIII.) was killed after the twelfth day, when it was apparently in perfect health. More extensive experience has enabled me to make some material alterations upon the few observations I made on these points in my last communication, (p. 163.) The most common morbid changes found in the lungs of those animals was a congested state of the blood-vessels of the lungs, and the effusion of frothy serum into the air-cells and bronchial tubes. In eight out of the seventeen these appearances were strongly marked. In some portions of the lungs the quantity of blood was so great as to render them dense. The degree of congestion varied in different parts of the same lung, but it was generally greatest at the most depending portions. This condensation was not unfrequently greater than what could be accounted for by mere congestion of blood in the vessels, and probably arose from the escape of the solid parts of the blood into the tissue of the lung. The frothy serum had frequently a more or less deep tinge of red. Some of those animals were opened immediately after death, which in three cases was hastened by a dose of prussic acid, given when the animal had become cold and stupid. In Experiments XVI. XX. XXII. and XXXII. though different parts of the lungs were much loaded with blood, the quantity of frothy serum found in the bronchial tubes was but trifling, and certainly not sufficient to impede the respirations to any great extent. In Experiment XVI. the animal lived fifty-two hours; and it is stated in my notes of the dissection, that there was a minute quantity of frothy serum in the larger bronchial tubes, and a small quantity of serum could be pressed from the minute bronchial tubes. A great portion of both lungs was so dense as not to crepitate when cut, was of a dark colour, and sank in water, though the cut surfaces were smooth, and did not present the granulated appearance of the second stage of ordinary pneumonia. In Experiment XX. the animal died between the seventy-sixth and eighty-third hour. Without giving the details of the dissection as they are written out in my notes, I may merely mention that part of the left lung was in a state of gangrene, and that some portions of the right lung were dark-coloured and dense, but floated in water. In Experiment XXII. the animal was killed after sixty-nine hours, and after it had become feeble. The lungs were dense, and there was only a small quantity of frothy serum in the bronchial tubes. In Experiment XXXII. the animal lived thirty-one hours. The chest was opened immediately after death, in fact, as soon as it had ceased to breathe. The lungs were dense and loaded with blood, but there was very little frothy serum in the larger and smaller bronchial tubes. In Experiment XI. the animal received a dose of prussic acid on the eighth day, after it was evident, from the state of its breathing, that it could not survive, and the chest was immediately opened. A great part of the left lung was in a state of pneumonia, in some places approaching the third stage, and many of the smaller bronchial tubes were full of a puriform matter. The right lung was dark-coloured and rather dense in some parts, but these when cut out floated in water. There was no appreciable quantity of frothy serum in the bronchial tubes of right lung. In Experiment XV. the animal died on the twelfth day. The left lung was voluminous, of a deep dark-colour, and dense. When cut into, little blood could be squeezed out, and the cut surfaces were not granular. The right lung was somewhat congested with blood at a few parts. The *trachea* contained some, and the left bronchial tubes much red mucus; while the bronchial tubes of the right lung were empty. In Experiment XXXIV. the animal was killed after four days and eight hours. The right lung was nearly healthy; while the left lung was almost entirely in the second and third stages of pneumonia, and there was very little serum in the bronchial tubes. Of the eight experiments in which the frothy serum was effused in great quantity into the bronchial tubes, the left lung was partly gangrenous in one, (Experiment XXI.) In other two experiments, XXXV. and

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* Valsalvae Opera cum Epistolis Anatomicis J. B. Morgagni, Epistol. Anatom. xiii. 36.—Supposing it to be proved that excitants applied to the mucous membrane of the lungs can excite sensation after section of the *vagi*, it would not necessarily follow that the impressions which give rise to these sensations are conveyed through the grey filaments proper to the ganglionic system, since it appears (Müller's Physiology, p. 664–672,) that a certain number of the white filaments of the cerebro-spinal system accompany all the branches of the ganglionic system.

XXXVI. part of the lungs was in the second and third stages of pneumonia, with effusion of puriform matter and much frothy serum into the bronchial tubes. In the seventeen experiments, therefore, five presented distinct traces of pneumonia, and in two it had run on to gangrene. In one of these the gangrenous portion was surrounded by circumscribed abscesses. In these experiments, I need scarcely again remark, great care was taken to secure a free passage to the air along the *trachea*, and this was apparently accomplished, except in Experiments XXXIII. and XXXVI. where the tracheotomy tubes employed were too small. One of the most important points to ascertain, in an investigation of this kind, is the first departure from the healthy state; to decide whether the effusion of the frothy reddish serum, by interfering with the usual changes of blood in the lungs, *causes* the congested state of the pulmonary blood-vessels and the laboured respiration; or whether this effusion is the *effect* of a previously congested state of the blood-vessels. If it be made out that the effusion of serum is consequent upon the congested state of the blood-vessels, we have next to inquire, what is the probable cause of this congested state of the pulmonary blood-vessels? In examining, with a reference to this question, the results of the experiments which we have made, one of the most important circumstances which presents itself is the fact, that the effusion of frothy serum into the bronchial tubes, in quantities sufficient to impede materially the respiration, is not a necessary consequence of division of the *vagi*, even when the lungs were found loaded with blood and when the respiration before death was very laboured. And this naturally leads us to doubt whether the frothy serum is the cause of the laboured respiration and the congested state of the pulmonary blood-vessels in those cases where it is present, though there can be no doubt that, when once it is effused, it must powerfully tend to increase the difficulty of the respiration and the impeded circulation through the lungs. Another important circumstance in enabling us to decide this interesting point is derived from the fact, of which I have satisfied myself after much careful examination, that this frothy fluid is not mucus, though it is occasionally mixed with it, but is the frothy serum so frequently found in cases where the circulation through the lungs has been impeded for some time before death. To investigate this point still farther, we killed several dogs at longer and shorter periods after section of the *vagi*, and we were more and more confirmed in the opinion, that the congestion of the blood-vessels is the first departure from the healthy state of the lungs, and that the effusion of frothy serum is a subsequent effect. Perhaps one of the most illustrative experiments I can select is the following.

Exp. XXXVII.—The recurrents and *vagi* were cut across and a portion removed in a middle-sized bull-dog at 2 o'clock P. M. At 7 P. M. his breathing was easy, and the respirations were 8 in the minute. Next day at 2 P. M. the respirations varied from 10 to 12. On the following day the respirations were 6 to 7, easy but heaving. On the third day at 11 A. M. the respirations were still between 6 and 7 in the minute, very heaving and prolonged, and the animal was evidently becoming very feeble. The femoral artery was exposed and a small branch opened, when the blood escaped in a feeble stream, and was almost as dark-coloured as venous blood. A fatal dose of prussic acid was now given, and the chest immediately laid open, care being taken to avoid the large vessels at the root of the neck. The large pulmonary vessels were now opened, and the blood both in the arteries and veins was dark and fluid. The lungs were not emphysematous, and contained much blood in their vessels. The left lung was of a deep dark-colour at the summit of the anterior lobe, and this when cut into was found quite dense, without granules, and scarcely any blood could be pressed from it. The bronchial tubes of this part contained some puriform mucus. A similar but smaller dense portion was situated about the middle of the same lung. Slices from these two portions sank in water. The right lung contained much blood; some parts of it were denser than others, but none of it sank in water. The mucous membrane of the bronchial tubes was moist and lined by a thin layer of mucus. A small quantity of frothy serum was found in some of the bronchial tubes. If the congested state of the blood-vessels precede, as we believe, the effusion of the frothy serum, we have next to inquire, what is the cause of the retardation of the blood, and congestion of the blood-vessels in the lungs? This, we were formerly inclined to believe, might depend upon paralysis of the muscular fibres of the bronchial tubes; but, being unable to obtain any satisfactory evidence of this, we again watched the phenomena more narrowly, and now believe that all the morbid changes observed in the lungs can be traced to the diminished frequency of the respiratory muscular movements. We have already dwelt at some length upon the influence of the *vagi* as exciters of respiration; and we have shown that when these nerves are tied or divided, the number of the respiratory muscular movements is instantly considerably lowered—generally more than one-half. Now, it is an established fact, that the flow of blood through the lungs is dependent upon the continuance of the respiratory process, and the great diminution in the activity of the respiratory muscular movements must be followed by a retardation and congestion of the blood in the lungs. This congestion of blood, as is well known, is generally followed by effusion of serum, and also predisposes the organs so circumstanced to various morbid changes, chiefly of an inflammatory kind. In the lungs this congestion is not only followed by the escape of the serum from the vessels, but also of the more solid materials, rendering the tissue dense. The effused serum is mixed up with the air moving along the bronchial tubes during inspiration and expiration, and it thus becomes frothy. A little blood also exudes from the congested mucous membrane, giving the serum a reddish tinge. Such, we believe, is the explanation of the morbid changes observed in the lungs after lesion of the *vagi*. If artificial respiration, which is a very imperfect substitute for the natural process, be carried on for some time in healthy lungs, a similar effusion takes place. Legallois, who must have been perfectly familiar with the appearance of this effused fluid, after stating that its formation was accelerated by artificial respiration in animals after section of the *vagi*, and after decapitation, is obliged to admit that a similar fluid is very frequently formed in animals not subjected to either operation when artificial respiration is carried on. "Car très-souvent il s'en forme un semblable dans les animaux entiers qu'on insufle." * We would not wish peremptorily to deny that the effects of the diminished frequency of the respiration may not be aided by paralysis of the muscular fibres of the bronchial tubes; we only state that after the most anxious endeavour to obtain some distinct evidence of this, we have been unsuccessful. Some may be inclined to believe that the *par vagum* may exert some favourable influence upon the capillary circulation of the lungs. The thing is possible, but we know of no well ascertained facts which could be adduced even as an analogical argument in favour of such an opinion.

I have not unfrequently observed in the human species, where the respiration has been impeded for some time before death, a condition of the lungs similar to that observed in the lower animals after division of the *vagi*. In several of the fever patients whom I have had lately occasion to inspect at the Royal Infirmary, the lungs in the posterior and middle parts were dark-coloured and gorged with blood and serum. When cut into, the substance of the lungs in some cases appeared at different parts denser than what could be accounted for by mere congestion of the blood-vessels, and a comparatively small quantity of blood could be squeezed from the cut surfaces, though the blood in other parts of the body was fluid; but sections of the lung generally, though not always, floated in water, and presented none of the granular appearance. In some cases the bronchial tubes contained a considerable quantity of frothy serum; in others very little. In all probability these morbid appearances are occasionally dependent upon the disturbed respiration consequent upon derangement of the central organs of the nervous system. I lately saw a gentleman labouring under fever, whose respirations for a short time were only 8 in the minute, though the lungs at the time were unaffected, but they fortunately soon rose to 16 in the minute, and he ultimately recovered. Dr Alison has suggested to me that these morbid changes in the lungs are sometimes owing to another cause. He believes that, in cases of fever, where the heart's action is feeble, the bronchiæ often somewhat obstructed, and the blood altered, the right side of the heart is unable to propel the blood through the lungs; it consequently goes on accumulating in their depending parts, and the same results follow as when the respiratory movements are diminished in frequency. In confirmation of this view he stated that he has seen these morbid changes occur in the lungs without any preceding diminution of the respiration. When we remember that the pulmonic circulation is dependent upon two distinct causes varying in efficacy—the contractions of the right side of the heart, and the chemical changes going on at the lungs—both of which are necessary for the proper propulsion of the blood through the lungs to the left side of the heart, we can easily understand how a diminution in the activity of the respiratory muscular movements, and the impaired contractility of the right side of the heart, should produce the same effect, viz. congestion of blood in the lungs and effusion of frothy serum.* I lately examined the bodies of two patients who died in the Royal Infirmary, which afforded a very important confirmation of the views which we have adopted regarding the cause of the morbid changes in the lungs after lesion of the *vagi*.

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* Experiences sur le Principe de la Vie, p. 242, 1812.

Case I. James Hallam, a sailor, aged 38, was admitted into the Royal Infirmary, on the 30th November last, with a slight attack of *bronchitis*, from which he rapidly recovered under the usual remedies. On the 6th December it was reported that he had no complaint. On the 11th it was remarked that he had been drowsy and almost constantly asleep since the day preceding, but he was easily roused, answered questions, and made no complaint. The pupils were contracted and little sensible to light. The respirations were only 5 in the minute and heaving. Some appearance of *anasarca*. Urine spec. grav. 1009, coagulable by heat and nitric acid; pulse of moderate strength. On the 12th the respirations were still only about 5 in the minute, heaving and prolonged, and exactly similar to what are observed in dogs after section of the *vagi*. On the 13th the respirations varied from 5 to 10, and were still heaving and prolonged. On the morning of the 14th the respiration remained of the same character. He had passed no urine since yesterday, and three ounces were drawn off by the catheter. He died at noon in slight convulsions.

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* The fluid state of the blood in fever will naturally aid in producing this congested state of the pulmonary vessels. Vide Magendie's Leçons sur les Phénomènes Physiques de la Vie, Tom. iv. 1838. As far as my observation goes the coagulability of the blood is not materially affected after section of the *vagi*.

Sectio cadaveris, 16*th*.—The arachnoid membrane appeared somewhat dry, and the convolutions were perhaps rather flattened. The vessels on the surface of the brain were well filled with blood, and a considerable number of red points presented themselves on slicing the brain. The substance of the brain was not firmer than natural, and there was little fluid in the ventricles. The heart was healthy, and the right side was filled with coagulated blood. There was a little reddish serum in the right side of chest. Lungs emphysematous along their anterior margins. The bronchial tubes were full of frothy serum, and the whole of the posterior and middle portions of both lungs were of a dark-red colour, and were full of blood and serum. Some parts of the lungs were so dense as not to crepitate when cut, but they presented none of the granulated appearance. The liver was natural, with the exception of the presence of an old cicatrix. The kidneys were of a yellowish colour externally, and their capsule was firmly adherent. The cortical structure of the kidney was nowhere apparent, and its place was occupied by a yellowish substance, which also encroached in some places upon the tubular structure. The kidneys were of their usual size, and firmer than natural. The trunks of the *vagi* were examined at their origin, at the middle and lower part of the neck, and within the thorax, and no morbid appearances observed.

CASE II. Peter Ballantyne, aged 40, a tailor, was admitted into the Royal Infirmary on the 14th December, with cough and expectoration tinged with blood. He also complained of palpitation, which was much increased by exertion. He stated that he had been subject to headache and vomiting, and his friends mentioned that he had been lately languid and indulged in long sleeps. When admitted the respirations were 14 in the minute. Urine scanty, specific gravity 1011, coagulable by heat and nitric acid. The pupils were contracted, and he appeared to have a tendency to drowsiness. After his admission the cough and expectoration diminished, but the strong action of the heart continued, and the urine retained its former characters. On the 18th the cough was slight; his respirations had fallen to five in the minute; pulse 96, small; countenance exsanguine; was very drowsy, but answered questions. On the 19th, he gradually verged into a state of coma. The respirations varied from 5 to 8 in the minute. The pupils were immovable and contracted. Pulse 84, rather feeble, but extremities warm. He had passed no urine for several hours. There was no œdema. No paralysis was observed during life. He died at 6 o'clock P. M.

Sec'io Cadaveris, 21st.—The surface of the brain was unusually pale; few red points presented themselves on slicing the brain, and the vessels appeared to be principally filled with serum. There were about two drachms of serum in the lateral ventricles, and half an ounce at the base of the brain. Two small clots of blood were found in the right *corpus striatum*, without surrounding softening. In the central portion of the anterior part of the *tuber annulare* there was a small cyst lined with a yellow membrane; and immediately posterior to this there was a small clot of blood about the size of a large pin's head. The large blood-vessels of the brain were dilated and thickened in their coats, but without calcareous deposit. The trachea and bronchial tubes contained a considerable quantity of frothy serum. The posterior and middle parts of the lungs were gorged with blood and frothy serum, and a considerable portion was so dense as not to crepitate when cut; but it did not present the granulated appearance, and floated in water. There was a mass of old tubercles in the apices of both lungs. The left ventricle of the heart was considerably thickened in its walls without diminution of its cavity. The valves were quite sufficient to fulfil their functions, and the thoracic aorta retained its elasticity. The kidneys were rather smaller than usual, and had numerous yellowish spots scattered through the cortical portion. The *vagi* were examined at their origin, at the lower part of the neck, and within the thorax, and no morbid appearance could be detected.

We were prevented by the friends in this, as in the former case, from examining the *vagi* through their whole extent; but this is perhaps a matter of little importance, as it was sufficiently evident from the symptoms that the drowsiness and slow respiration in both arose from cerebral derangement. In the first case, this in all probability arose from the accumulation of urea in the blood, exerting a deleterious influence upon the brain—a not unfrequent concomitant of kidney disease.* It may be a matter of doubt whether the cerebral derangement in the second case arose from the same cause, or from the effusion of blood. The symptoms certainly presented themselves in a more gradual manner than what we should expect in cases of apoplexy, and more resembled the coma which occurs in kidney disease. I may state that Dr Christison has seen cases of coma in kidney disease where no œdema was present.

We have embraced every opportunity of examining the effects of lesion of the *vagi* upon the secretion of mucus from the inner surface of the bronchial tubes. We have already stated that we believe the frothy fluid so frequently found in the bronchial tubes is not mucus and the result of a morbid secretion, as Wilson Philip[*] and others have imagined, but that it is chiefly, and in some cases almost entirely, composed of serum, and is consequent upon the congested state of the blood-vessels—an effect so frequently observed in other parts of the body. In killing animals at very various periods after section of the *vagi*, we have never found the inner membrane of the bronchi unusually dry, but always covered with the usual quantity of protecting mucus, except in those cases where inflammation was present. And we can explain how the application of galvanism in the experiments of Dr W. Philip was so efficacious in preventing this effusion, without being obliged to admit that the secretion of the mucous membrane is deranged; for, since the galvanism was applied so as to keep up a constant spasmodic action of the limbs, the frequent contractions of the respiratory muscles produced by this excitation would prevent the diminished frequency of the respiratory movements. The explanation given by Brachet[†] of the source of this frothy fluid—viz. that it is the usual mucous secretion of the lining membrane of the air-passages which has accumulated there, because this membrane has lost its sensibility, and the muscular fibres of the bronchi have lost their contractility,—is so palpably objectionable as to require no remarks.

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* Vide Christison on Granular Degeneration of the Kidneys, Edinburgh, 1838, p. 92.

An excellent illustration of the numerous difficulties with which the physiologist has to contend, from the impossibility of insulating any individual organ from its mutual actions and reactions with others, when he wishes to examine the order and dependence of its phenomena, is furnished by the experimental history of the *par vagum*. The two first experimenters, Rufus of Ephesus,[‡] and Galen,[§] attended only to the loss of voice. A new direction was then given to this investigation by the suggestion of Piccolhomini[||] that it affected the heart's action, and numerous experiments were afterwards adduced in support of this opinion. Some described its effects as instantly fatal. F. Schrœder stated, that, when both nerves are tied, the animal dies instantly "animal vitam protinus amittit."[¶] Bohnius, in mentioning an experiment of this kind, indulges in a highly figurative expression, which appears to be still a favourite with some physiologists, and describes the animal dying as if struck with a thunderbolt, "fulmine quasi tactum."[**] Varingnon relates, that when he had tied these nerves in a cat, the animal "était mort dans l'instant sans aucun mouvement d'aucune partie de son corps."[*] Some experimenters, however, such as Willis, Vieussens, and many others, found that the operation was not always instantly fatal, and that the animal might live twenty-four hours, and even several days: and though puzzled to account for this on their favourite theory—that a constant supply of animal spirits flowed along the vagus to the heart—yet they succeeded in accommodating the facts to this explanation, by supposing that a diminished supply of the vital spirits continued to reach the heart through the more circuitous and less favourable channel of anastomoses.[†] Little progress was made in this inquiry for a long series of years, and the prevailing doctrine appears to have been, that section of the nerves, some way or another, affected the heart's action. The experiments of Dupuytren[‡] are chiefly important, as they directed the attention of physiologists in an especial manner to the derangement of the lungs. No doubt the derangement of the respiratory function had been previously remarked by Valsalva, Morgagni, Molinelli, Haller, Brunn, Cruickshank, and some others. Valsalva states, however, that nothing morbid was found in the chest in a dog which he dissected after death from this operation.[§] Morgagni found the lungs congested with blood.[‖] Mr Cruickshank, in Experiment II. where the animal died on the seventh day after division of the second *vagus*, found every organ healthy except the lungs, and these he describes to have been of a red-brown colour, so dense as to sink in water, and containing frothy fluid with some puriform matter in the bronchial tubes. In Experiment III. he found the lungs loaded with blood.[¶] Dupuytren concluded from his experiments,—and in this Hallé and Pinel, the reporters on the memoir, concurred,—that animals in which the *vagi* are cut die of asphyxia, because the atmospheric air, although continuing to penetrate freely into the lungs, can no longer combine with the blood, since this combination cannot take place except under the agency of the nervous system. This is also the opinion which Sir Astley Cooper is disposed to adopt in the account of his experiments on the *vagus*, given in Guy's Hospital Reports, already referred to.[**] Soon after the publication of Dupuytren's memoir, it was shown by the experiments of Dumas and

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[*] Inquiry into the Vital Functions, Chapter XII.

[†] Oper. cit. p. 160.

[‡] Vide Morgagni de Sedibus et Causis Morborum, Epist. XIX. Art. 23.

[§] De Locis Affectis, Lib. I. Cap. 6.

[||] Anatomicæ Prælectiones Arch. Piccolhomini, Romæ, 1586, p. 272.

[¶] Addit. ad Vesling. Synt. c. 10, n. 7, as quoted by Morgagni.

[**] Circul. Anatom. Progym. vi. p. 104, Lips. 1686.

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[*] Histoire de l'Acad. Roy. p. 28, 1706.

[†] Willis Cerebri anatome, p. 324; Vieussens Neurographia Univer. p. 179, Ludg. 1785.

[‡] Biblioth. Med. Tom. xvii. p. 1. An abridgement of the memoir is given in the Journal Medec. de Corvisart. Tom. xiv. p. 45.

[§] Valsalvæ Opera, &c. Epist. Anatom. xiii. p. 512.

[‖] Oper, cit. Epistol. Anat. 29.

[¶] Medical Facts and Observations, Vol. vii.

[**] No. III. p. 470.

De Blainville in France, and of Emmert in Germany, that arterial blood may continue to circulate in the arteries after section of the *vagi*. Dumas proved that the arterial colour could be restored by artificial inflation of the lungs after the blood in the arteries had become venous.[*] Emmert satisfied himself that, after the trachea had been compressed in rabbits and cats in which the *vagi* had been divided, the arterial hue of the blood could be restored by removing the pressure, and allowing the air again to enter the lungs.[†] It was likewise proved by Sir B. Brodie in this country, that the usual chemical changes take place at the lungs in a decapitated animal when artificial respiration is kept up.[‡] Provençal in France also showed that an animal in which the *vagi* have been divided continues to deteriorate the air by the formation of carbonic acid gas, though not to the same extent as before the operation.[§] The experiments of Le Gallois[‖] at last came, and satisfactorily explained all the anomalies connected with the occasionally rapid death of the animal, and the sudden arrestment of the chemical changes at the lungs, which had so long puzzled physiologists. He pointed out by decisive experiments, that when the *vagi* are divided, more especially in young animals, the superior aperture of the larynx frequently becomes closed, and the animal is suffocated. He also fully described the congested state of the blood-vessels of the lungs and the effusion of frothy fluid. Legallois, however, seems to have imagined that the effused frothy fluid was the cause of all the dyspnœa, and that this effusion in its turn was dependent upon a kind of paralysis in the lungs. The same morbid appearances in the lungs pointed out by Legallois have been frequently observed and described by more recent experimenters, among whom we may mention Dr Wilson Philip,[¶] Dr Holland,[**] and M. Brachet.[††] In examining the morbid appearances in the lungs, we have observed no facts in confirmation of the statement of Mr Swan, that section of the vagi produces emphysema of the lungs.[‡‡] We have undoubtedly seen portions of the lungs emphysematous in a few cases, but we had no reason to believe that this was the necessary result of the section of the nerves. That the lungs should collapse imperfectly when gorged with blood and frothy serum, is certainly no proof that they are emphysematous. Neither have we seen any thing confirmatory of the opinion of Mayer, * that if the animal live more than forty-eight hours, the arteries and veins of the lungs, even to their minute ramifications, as well as the cavities of the heart, are filled with white, firm, and compact coagula, consisting of the fibrine and albumen of the blood; while if the animal die sooner, the coagula are soft and dark-coloured. It is these coagula, according to Mayer, which cause the arrestment of the movements of the heart and produce death. He believes that when the influx of the nervous influence is arrested by division of the vagi, the fluidity of the blood ceases, and it separates itself into its constituent parts, as when drawn from the body. Müller has been equally unsuccessful in finding these fibrinous clots in the pulmonary vessels. †

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[*] Journal General de Médecine, Tome xxxiii. p. 353.

[†] Archiv. für Physiologie von den Professoren Reil und Autenrieth. Neunter Band. S. 380, 1809, und Eilfter Band S. 117, 1812.

[‡] Philosophical Transactions, 1811.

[§] Bulletin des Sciences Medec. Tome v. p. 361.

[‖] Sur le Principe de la Vie, 1812.

[¶] Oper. cit.

[**] An Experimental Inquiry, &c. Edinburgh, 1829.

[††] Oper. cit.

[‡‡] Essay on the connection of the Heart and Arteries, &c. 1829.

The explanation of the fact, that lesion of one *vagus* does not necessarily nor even generally induce disease of the lung of that side, may, I believe, be satisfactorily obtained, when we consider that this is not likely to diminish the number of respirations so much as the lesion of both *vagi*. We have in several dogs cut the *vagus* of one side, and after having observed the changes produced, we then divided the *vagus* of the opposite side. Some of the results will be found in the following table.

	Respirations before operation.	One vagus divided.	Both vagi divided.
Exp. 1st,	14 to 16	11 to 12	5 to 6
— 2d,	15	13	6 to 7
— 3d,	16 to 17	9 to 10	6 to 7
— 4th,	16 to 18	12	8

In other two experiments the frequency of the respirations was not sensibly affected by section of one *vagus*, though the animals were kept for eight days. In one of these the other *vagus* was cut, and the frequency of the respirations suffered a very marked change, falling from 20 to 8 in the minute.

In examining the effects of lesion of the *vagi* upon the lungs, it is particularly worthy of remark, that these morbid changes in the lungs do not necessarily follow the division of both *vagi* and recurrents. In a dog, Experiment XIII. which was killed in presence of Dr Alison, twelve days after the section of the *vagi* and recurrents, the lungs were found perfectly healthy, though the respirations before death were still slow and heaving. This animal had recovered from the effects of the operation on the digestive system, and was rapidly gaining flesh and strength. In dissecting the nerves after death, the cut ends of the left *vagus* were one inch and one line apart; and the distance between the cut ends of the right was one inch and two lines. In another dog, Experiment X., which died apparently from inanition eight days after division of the *vagi* and recurrents, the lungs were also found healthy. The nerves in both of these animals were carefully examined after death, and no anormal arrangement of them could be observed. Previous to the performance of Experiment XIII. I fully believed in the correctness of the opinion expressed by Magendie* and by Müller, † that the simultaneous division of both *vagi* is always fatal within a few days. I have, however, no doubt in my own mind, and this was the opinion of all who saw this animal after the tenth day, that he had fairly recovered from the operation. About the sixth and seventh days after the operation, he was so weak as to stagger occasionally in walking, while at the time of his death he was so lively and active as to leap over places between three and four feet high, for his amusement. I afterwards regretted that I did not permit him to live longer, but I then had hopes that two others would survive the operation. In this I was, however, disappointed. I find that M. Sédillot, in detailing an experiment, (to which I shall have occasion again to refer,) where the animal lived two months and a-half after section of both *vagi* with loss of substance, states, that there was no induration of the lungs, and that they contained little blood. In examining the details of the very numerous experiments which have been made upon the effects of the division of both *vagi*, we shall find that by far the greater majority of animals die before the completion of the third day. In the seventeen experiments we have so frequently referred to, eleven died before the completion of the third day, and seven of the eleven before the completion of the second day. Valsalva remarks that animals in which the *vagi* are simply divided, live longer than those in which the nerves are tied.‡ In one of Petit's experiments the animal lived seven days.§ One dog experimented on by Baglivi died on the twelfth day; and another on the seventh day.‖ Morgagni details, in his additions to the works of Valsalva, the results of two experiments on dogs—one of which lived to the tenth, and the other to the eighteenth day.¶ The eighteenth day is apparently the longest period to which he had seen an animal live after section of the *vagi*, as he refers to it with this view when speaking of the duration of life after this operation. None of the animals experimented on by Baglivi and Morgagni ever recovered from the operation, but gradually became weaker. Besides, we are led to believe, that the nerves were simply cut across without loss of substance. M. Dupuy, in his experiments, found that horses lived to the fifth, sixth, and seventh day, when care was taken to admit a sufficient quantity of air into the *trachea.*[*] De Blainville, in giving the results of his experiments upon pigeons, informs us, that these animals died on the sixth or seventh day.[†] Sédillot states that, in one experiment, a dog lived twenty-one days; and that another dog lived two months and a-half after the operation, though more than an inch of the nerve was removed on both sides[‡] Arnemann has detailed an experiment at considerable length, where the animal appears to have completely recovered from the effects of the division of both *vagi* with loss of substance.[§] A portion measuring rather more than four lines was removed from the right, and one of eight lines from the left *vagus.* The animal at first suffered very severely from the operation, and it is described as gasping for breath. After a week it began to improve gradually, though the breathing was still difficult. After a month and a-half it had wonderfully improved, ate voraciously, but did not become fat. From the description given by Arnemann of the condition of the nerves in this animal when dissected after death, and from an engraving of the right *vagus,* (Tab. III. Fig. XVIII.) from which a smaller portion was removed than from the left, it appears that there was no regeneration of the removed portions of the nerves. The preparation of the right *vagus* was in the possession of Blumenbach. Notwithstanding the minuteness of the details of this case, and the apparent care with which the experiment was performed, and the nerves examined after death, we find that some authors express great suspicion of its accuracy.[||] It would appear that Arnemann had seen several dogs recover completely after division of both *vagi*, and that the animals were afterwards employed in other experiments upon the regeneration of some of the large nerves of the limbs; but I must confess that the statement is somewhat startling.[¶] M. Brachet states in a note, that M. Fouacade had in 1820 presented to the Académie de Médecine, a dog in which four lines had been removed from the left

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* Tiedemann's Zeitschrift für Physiologie. Zweiter Band, S. 74–5, 1826.

† Elements of Physiology, Vol. i. p. 358. Baly's translation, 1838.

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* Milligan's translation, p. 398, 1831. † Baly's translation, Vol. i. p. 356.

‡ Oper. cit. Epist. Anatom. xiii. 23 and 38.

§ Memoires de l'Acad. Roy. A. 1727, Exper. 2, p. 6.

‖ Opera omnia, p. 676–7, Exper. 7 and 8.

¶ Oper. cit. Epist. Anat. xiii. 36–7, Tom. i.

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[*] Journal de Medecine, Chirurgie, &c. Tome xxxvii. p. 356. December 1816.

[†] Nouv. Bullet. de la Soc. Philom. Tome i. An. 2, p. 226.

[‡] Thèse du Nerf, Pneumogastric, &c. Exper. 1 and 6, Paris, 1829. M. Sédillot also states, (p. 23,) that M. Begin kept a dog alive for a month after division of both *vagi*. I find that M. Chaumet states (Essai sur la Physiologie de l'Estomac, p. 17, Paris, 1828,) that this animal operated on by Begin lived thirty-three days, and that no very obvious change was observed in the digestive process. M. Chaumet farther adds, that, in his own experiments, a dog lived fourteen days, and digested.

[§] Versuche über die Regeneration der Nerven. hundert und zehnter versuch. S. 99, Göttin. 1787.

[||] Emmert. Oper. cit. Eilfter Band S. 420. Lund, Physiologische Resultate der vivisectionen neuerer Zeit. S. 241.

[¶] Ich habe *mehreren* starken hunden *beide Vagos* und einen Intercostal-nerven zu gleicher Zeit durchschnitten. Nach einen *Monate* durchsnitt ich eben diesen Thieren mehrere grosse nerven beider *Vorderbeine* und in der *Folge* nach beide *Ischiadische Nerven* von *allen diesen Thieren* habe ich kein einziges verloren, Oper. cit. S. 193.

vagus, and the same operation had been repeated on the right a few days after.[*] On this case M. Brachet remarks that, supposing the nerves to have been accurately divided, the free anastomoses between the laryngeal nerves at the *larynx* may have sufficed to transmit the nervous influence. In our experiment the recurrents were also divided.

Gastric Branches of the Vagus.

Are the gastric branches of the vagus partly nerves of motion? In my former communication (p. 150) it was stated that I had repeatedly seen muscular contractions of the œsophagus induced by irritating the trunk of the nervus vagus in the neck, extend also over the cardiac extremity of the stomach. In the stomach they were evidently slower, more prolonged, and vermicular than in the œsophagus, and they extended somewhat slowly from the cardiac orifice over a greater or less extent of the left portion of the stomach. Irritation of the vagus frequently fails to produce these movements in the stomach, even where the muscular fibres of the œsophagus are thrown into vigorous contractions. These muscular movements of the stomach from irritation of the vagus, have been frequently observed by Tiedemann and Gmelin.[†] They have also been inferred by Breschet and Milne-Edwards from the effects upon the digestive process of galvanizing the lower ends of the cut *vagi* in the neck of a living animal.[‡] Mr Mayo, however, could not perceive any muscular movements of the stomach upon irritating this nerve;[§] and the same thing is very strongly stated by Müller.[‡] I have again carefully and repeatedly performed this experiment, and I am confident that though it frequently fails, yet it often succeeds. These muscular movements are, as I have already stated, different in their character from those of the œsophagus, and resemble more those of the intestines. I have been unable to satisfy myself whether the muscular movements seen on irritating the vagus are dependent upon any direct excitation of the muscular fibres of the stomach through the filaments of the vagus distributed upon it; or whether they are dependent upon the movements of the muscular fibres of the lower part of the œsophagus being propagated onwards in the same manner as we find that contractions excited in the muscular fibres of the intestinal tube are propagated along it to a greater or less extent, and apparently without the aid of nerves. The supposition, that the movements of the stomach resulting from irritation of the vagus are merely propagated onwards from the œsophagus, is favoured by the circumstances, that the stomach frequently remains quiescent when the œsophagus is thrown into vigorous contractions; and that when these movements of the stomach do occur, they commence at the cardiac orifice, and extend themselves in a vermicular manner over the left extremity of that organ. I have likewise seen similar movements of the stomach produced by throwing the lower part of the œsophagus into contraction by pinching it with the forceps; but it is, however, quite possible that some of the gastric branches of the vagus were also included between the blades of the forceps. With the view of elucidating this point, I performed the following experiments upon a dog and three rabbits. In these animals the vagi were exposed in the neck, but without injuring them, from two to five hours after a full meal, and the stomach was then exposed by cutting through the parietes of the abdomen. I intended to have cut the *vagi* after watching the movements of the stomach, and to have observed the effects of the section upon those movements. The muscular movements of the stomach were, however, in all of these animals so indistinctly marked, that I could obtain no satisfactory results; and, as the experiment was a cruel one, I did not persevere in it. That muscular movements may occur in the stomach after section of the *vagi*, sufficient to propel the chyme onwards into the *duodenum* is, we conceive, sufficiently proved by the experiments which we shall immediately have to examine in detail, where the lacteals were found full of chyle after section of both pneumogastrics in the neck. It is the opinion of Breschet and Milne-Edwards, * and of Brachet,† that lesion of the vagi interferes with the proper performance of the digestive functions, by arresting the movements of the muscular fibres of the stomach; in consequence of which the aliments always preserve the same relation, and cannot be transformed into chyme except at the surface, or that part in contact with the walls of the stomach. Tiedemann and Gmelin also believe that the movements of the stomach principally depend upon the influence of the vagi. ‡ That the gastric branches of the vagus are not partly motor we will not deny, but of this we have not been able to obtain any decided evidence. It, however, cannot be to the extent maintained by the authors we have mentioned, from facts already stated.

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[*] Oper. cit. p. 167. This experiment is also referred to by M. Sédillot, Oper. cit. p. 23.

[†] Recherches Experimentales Physiol. et Chem. sur la Digestion, &c. traduites par Jourdan, p. 374.

[‡] Archiv. Gen. de Med. Tome vii.

[§] Anatomical and Physiological Commentaries, No. ii. p. 15.

[||] Elements of Physiology, translated by Baly, 549.

Effects of Lesion of the Vagi upon the Functions of Digestion.—That lesion of the *vagi* is generally followed by vomiting (in those animals susceptible of it,) loathing of food, and arrestment of the digestive process, has been incontrovertibly

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* Oper. cit. Tome vii. p. 197.

† Oper. cit p. 204–5.

‡ Oper. cit. p. 374.

proved by numerous experimenters. That perfect digestion n
occasionally take place after division of the vagi in the ne
even when the cut ends of the nerves are kept apart from e
other, we are also fully convinced. In four of the sevent
animals experimented on for the purpose of examining the mor
changes in the lungs, we obtained sufficient evidence of the res
ration of the digestive process. The most satisfactory of th
was the subject of Experiment XIII. to which we have alrea
more than once referred. The animal operated on was a pre
large, full-grown, and lean bull terrier. Both recurrents and v
were divided, and a portion of each removed. Immediately af
the operation the respirations were much diminished in frequen
and continued so until its death, but were quite easy, unless wh
the animal was made to struggle. The nerves were cut at ha
past eight o'clock, A. M. on the 15th June, and at three P. M.
the same day he ate some bread and milk, which he soon af
vomited. He appeared to suffer no uneasiness, but was dull a
languid. On the afternoon of the 16th, he was prowling abo
apparently for food, and ate voraciously when he obtained it ; l
it was soon after vomited unchanged, except that it was mi
with fluid. He also drank freely. He appeared pretty live
On the 17th, he still ate and vomited the food unchanged. (
the 18th, vomited some animal food unchanged, which had be
retained for at least seven hours. On the 19th, vomited the fo
taken in the morning, and again swallowed it. 20th, No vom
ing to-day and food retained. 21st, Ate voraciously and appare
ly with relish, but food is vomited, and again partly swallowed.
the evening appeared duller than he had been for the two p
vious days. On the 22d was less lively, and appeared weak,
having a considerable difficulty, when lying, in getting upon
legs, and occasionally staggering when walking. Almost const
cough. No vomiting to-day, except of some bread and m
about a minute after it was taken, which he again immediately
up and retained. 23d, Less feeble than yesterday ; still so
vomiting, and the animal food vomited was partly in a pulpy st
but without fœtor. 24th, Less appetite to-day, and some vom
ing of animal food in a state of pulp. Is, however, regaini
strength, and can walk, and even run without staggering. 25
ate voraciously to-day. Still occasional vomiting of half-diges
food. Is rapidly gaining strength, and has less cough. 26
Has eaten largely of animal food ; no vomiting. 27th, T
morning was very lively and active, and prowled about for fo
and water when liberated from the room in which he was conf
ed ; and leaped over a place between three and four feet high,
parently for his amusement. Barks, but very feebly. At ha
past 11 A. M. he was fed with three-quarters of a pound of sw

butter, a small part of which he immediately vomited, and again swallowed. After this he made no efforts to vomit. About half-past 3 P. M. he was killed by a dose of prussic acid, in the presence of Drs Alison and Duncan and Mr Spence, and the abdomen was immediately laid open. After the effects of the prussic acid began to manifest themselves, he vomited some of the butter in a fluid state. *Sectio.* The mesentery presented a beautiful display of innumerable lacteals filled with a white milky fluid ; the thoracic duct was also full of chyle. Part of the butter, altered in its appearance, was found in the stomach and small intestines. The mucous membrane of the stomach presented a blush of red in the splenic extremity, and was of its natural thickness and consistence. The nerves were then carefully exposed in the neck, and it was ascertained by measurement, that between the cut ends of the left vagus, when the neck was not stretched, the distance was one inch and one line ; and between the cut ends of the right vagus it was one inch and two lines. The distance between the cut ends of one recurrent was one inch, and between those of the nerve of the opposite side it was somewhat less. The cut ends of the *vagi* formed little bulgings, and there was no regeneration (as was to be expected) of the nerves. The lungs crepitated everywhere, and contained very little blood. Some adhesive mucus was found in several of the larger and smaller bronchial tubes. In one portion of the lung, which was about an inch in length and half an inch in breadth, the bronchial tubes appeared full of this adhesive mucus. The sympathetic was dissected in the chest, and, compared with a similar dissection in two dogs, was nearly of the same size. The nerve of the left side appeared somewhat larger than that of the opposite side, and also larger than the corresponding nerves in the other two dogs, but this was not to any great extent. A portion of the mesentery, in which the chyle has been retained in the lacteals, and also the neck of the animal, showing the distance between the cut ends of the nerves, have been preserved.

We shall give some short details of the other three experiments. One of these was performed upon a full-grown middle-sized terrier bitch, and has been already referred to under Experiment X. Both *vagi* and recurrents were divided, and a portion removed. The frequency of the respirations was diminished to one-half immediately after the division of the *vagi*, but they were performed with ease. The nerves were divided on the 22d of June, at 8 o'clock A. M. At 10 A. M. she appeared very dull and languid ; and at 4 P. M., when solicited to take food, refused to eat. On the 23d, took some bread and milk, which were soon after vomited. 24th, Took some bread and milk at half-past 10 A. M., which had not been vomited at 4 P. M. Part of it was vomited in the evening, but was scarcely changed. 25th, Took some animal food at 11 A. M. Part of it was vomited unchanged between 3 and 7 o'clock P. M.; the rest was retained. 27th, Vomited part of the animal food taken yesterday, which was softened on the surface without fœtor. 28th, Is more lively, looks about her when she expects food, and eats with apparent relish. Part of the animal food (liver) taken yesterday had been vomited, and was partially digested,—some of it being reduced to a brownish fluid; other portions were reduced to a pulp on the surface. All of it was without fœtor; and the altered portion permanently reddened litmus paper placed for a short time in contact with it. On the 29th and 30th, and the 1st of July, had very frequent vomiting of food, which she again swallowed only to vomit the greater part of it again. It was evident, however, that more food was swallowed than what was rejected by vomiting. Was obviously becoming weaker, and was very lean. On the 2d July was found dead at 8 A. M. The central parts of the body were still warm, and the abdomen was immediately laid open. *Sectio.*—A few lacteals were observed on the mesentery, some of which were punctured, and a thick white fluid flowed out. The thoracic duct was then exposed, and found full of chyle, and a ligature was placed around it at the upper part of the chest. Another ligature was also placed around it at the lower part of the chest, and the duct punctured below it, when a thick white fluid flowed out. The nerves were then exposed in the neck; and it was ascertained that the distance between the cut ends of the *vagus* on one side was one inch two lines, and on the other side seven-eighths of an inch. The distance between the cut ends of the recurrents was at least one inch. The parts were left in this condition until they had been examined by Drs Alison, Knox, Craigie, Handyside, Duncan, and Mr Fergusson. The mucous membrane of the lungs was pale, and covered by a thin layer of mucus. There was no effusion into the air-cells, and the lungs were everywhere spongy, and contained very little blood. The stomach contained a considerable mass of hair, but no food; and its mucous surface was quite healthy. No softness or redness of cut ends of nerves. The preparation of the neck and the thorax, showing the distance between the cut ends of the nerves, and the thoracic duct full of chyle, is preserved in the University Museum. The subject of another experiment was an old and large pointer, already referred to under Experiment XI. The *vagi* were cut on the 27th June, at half-past 8 A. M. On the 28th he was dull and languid; had no apparent uneasiness; refused to eat, and had a little vomiting. On the 29th, took a little food, which was soon after vomited. On the 30th, was very dull, and vomited the small quantity of food taken. 2d July. Took a quantity of milk, which some time after was vomited in a coagulated state. 3d, Appetite deficient, and the food taken was in great part again vomited. On the 4th the appetite had considerably improved; but a great part of the food taken was vomited after a longer or shorter time. On the 5th, still vomited a great part of the food taken, and was evidently becoming more feeble. At 3 P. M. the state of his breathing showed that he was evidently beyond recovery; and he was poisoned by a dose of prussic acid. After the acid had begun to produce its effects, he vomited some half-digested bread and milk. *Sectio.*—The abdomen was immediately laid open in the presence of Dr Patterson of Leith, and we were shortly after joined by Dr Knox. Several lacteals were seen upon the mesentery full of chyle. Some of these were punctured, and a white thick fluid flowed out. The thoracic duct was now exposed, and found full of chyle. The pneumogastrics were then dissected in the presence of these gentlemen; and it was ascertained by measurement, that the distance between the cut ends of the nerves was seven-eighths of an inch on one side, and one inch and one-eighth on the other, when the neck was in a semi-bent position. The stomach was quite healthy, and slightly red on the inner surface, as is usual when digestion is proceeding. The lungs, particularly the left, were considerably diseased, as we have already described under the head of morbid changes in the lungs. The fourth and last experiment was performed upon a full-grown and large mongrel, (betwen a pointer and terrier,) the subject of Experiment XV. The *vagi* and recurrents were divided on the 19th June, at 3 P. M. The respiration was easy after the section of the nerves. On the 21st, vomited some liver taken the day before, which presented no decided change. Is more lively. At 7 P. M. ate some rabbit's flesh with apparent relish, but vomited it soon after. On the 22d, at 4 P. M., vomited unchanged some rabbit's flesh taken at 8 A. M. Is still languid and dull. On the 23d, coughed pretty frequently, and vomited all, or nearly all, the food taken. 24th, At 10 A. M., walked across the room to reach some bread and milk, of which he ate a considerable portion. Is generally dull and listless. At 4 P. M. he had vomited part of the bread and milk. 25th, Is more lively; still coughs. Ate some liver at 10 A. M., which was not vomited at 3 P. M. On returning at 6 P. M. I found part of it had been vomited, and this was decidedly converted into a pulp on the surface. 26th, Took a considerable quantity of animal food (sheep intestine) with apparent relish. Some of it was vomited after eight hours, and was partly soft and easily lacerated, and partly reduced to a pulp, without emitting any disagreeable odour. 27th, Still vomits part of his food in various stages of digestion. 29th, Vomited some liver in a pulpy state, not putrid, and reddening permanently litmus paper. Very frequent cough, and discharge of mucus from the nostrils. 30th, Eats little, and vomits that little. In the evening refused food, and was found dead next morning. *Sectio.*—The distance between the cut ends of one *vagus* was one inch and two-eighths, and between those of the opposite side one inch. The distance between the cut ends of the recurrents was one inch and-a-half. The left lung was very considerably diseased, as we have already described.

We believe that no one who examines the details of the first of the four experiments we have here related, can for a moment doubt that the digestive process was fully re-established before the death of the animal. Though the contents of the lacteals were not chemically examined, every one conversant with the recent history of physiology knows that late experiments have fully proved that the lacteals absorb none of the ingesta in any great quantity, except they have been previously converted into chyme in the stomach. Besides, as the animal was enclosed in a room where nothing vomited could escape detection, I am confident that considerable quantities of food which he had taken into his stomach, during the last few days of his life, had disappeared from the digestive tube. Above all, the animal was undoubtedly gaining rapidly in flesh and in strength; and what is digestion but the name given to those actions by which the ingesta are converted into materials fit for ministering to the nutrition of the body? In the second experiment, besides the evidence afforded by the vomiting of animal food partially digested, and capable of permanently reddening litmus paper, we discovered the presence of chyle in the lacteals after death. I am also equally confident that a considerable quantity of food had been retained in the stomach, notwithstanding the frequency of the vomiting; and this had disappeared from the digestive tube. In the third experiment the milk was vomited in a coagulated state, and chyle was also found in the lacteals and thoracic duct. In the fourth experiment we had also what I cannot regard but as unequivocal evidence of digestion, viz. the vomiting of half-digested food permanently reddening litmus paper, and the disappearance of a considerable quantity of food from the digestive tube. In the experiment of Arnemann, to which we formerly referred, digestion must have been re-established, since the animal lived 165 days. In the experiment mentioned by Sédillot, digestion must at least have been partially established, otherwise the animal could not have lived two months and a half. It is of importance to remark, that we only obtained distinct evidence of digestion in the four animals which lived beyond the fifth day, and that even in these the digestive process was at first completely arrested and only gradually improved. In the other thirteen experiments the animals either refused to take food; or the food when taken was either vomited or remained in the stomach unchanged. We believe, then, that we are justified in concluding, that a deleterious influence is propagated downwards to the stomach by lesion of the *vagi*, yet that, if the animal live for a certain time, the digestive process may be re-established. Leuret and Lassaigne have detailed an experiment where digestion proceeded immediately after section of both *vagi* with loss of substance. They removed from three to four inches of each pneumogastric in a horse in perfect health, after fasting for four days. *Tracheotomy* was performed. After the operation the animal eat with appetite, and a ligature was then tied around the *œsophagus*. The animal was killed eight hours after eating. The stomach contained about the half of what it had eaten; the rest had passed into the small intestines. That which remained in the stomach was chymified, as analysis proved. The lacteals on the mesentery were distended with chyle, which was found by analysis to consist of the usual ingredients. The thoracic ducts (in the horse there are two) were filled with chyle. M. Dupuy was present, and renounced his previously published opinions, "J'en conviens a-t-il dit je m'etais trompé."[*] They also state that the same experiment was repeated with the same results.[†] Paetsch, in an inaugural dissertation,[‡] transcribes part of a letter from Augustus Schultze, then Professor of Physiology at Fryburgh, in which it is stated that he fed two dogs with milk, and in one of these he cut the *vagi* on the *œsophagus*.[§] Both were killed an hour and a half after. In the dog operated upon, the milk was digested, and the small intestines contained chyme, and the lacteals were full of chyle. The digestion had not advanced so far in the dog not operated upon. He states that he satisfied himself by subsequent dissection that all the filaments of the nerves had been cut. From this single comparative experiment he rashly concludes that section of the *vagi* does not retard, but, on the other hand, accelerates the digestion by the irritation of the intestines. He further states, that he repeated this experiment with the same results, only the digestion had not proceeded so far.

Magendie states, that in experiments in which the *vagi* were cut upon the *œsophagus*, so as to avoid the disturbance of the respiration consequent upon section of the *vagi* in the neck, he found that the digestion was not arrested. Brachet objects to these experiments, that no one can be certain that all the filaments of the *œsophageal plexus* are divided, unless the *œsophagus* be at the same time cut across; and besides, Magendie does not state that he satisfied himself of the division of all the filaments by dissection after death. Brachet affirms, that, on repeating this experiment, and taking the precaution of cutting the *œsophagus* across, he found that perfect digestion was prevented.[*] Paetsch in some experiments, cut the *œsophagus* across where it perforates the *diaphragm*, and found that the digestion of milk previously introduced into the stomach, was arrested.[†] That digestion should be arrested for a time after the serious injuries inflicted in performing such experiments, is nothing more than what we should expect; and the more especially when we remember the instructive experiments of Brachet, where he found that the mere incisions necessary for laying bare the *vagi* in the neck, though the nerves themselves were left uninjured, were sufficient, in some cases, to arrest the digestion of food previously taken into the stomach. Many experimenters, among whom we may enumerate Haller,[‡] Brunn,[§] De Blainville,[||] Dumas,[¶] Dupuy,[**] Legallois,[††] Macdonald,[‡‡] Wilson Philip,[§§] and Dr Hastings,[||||] have never observed evidence of digestion after lesion of the *vagi*; but such negative experiments cannot be considered at variance with the positive experiments we have stated; they only show, what every physiologist who has experimented much on this subject must, I think, be obliged to confess, that the digestive process is generally arrested after section of the *vagi* for the short time the animal usually lives; but they can never overthrow the results derived from positive experiments, provided these have been accurately performed, and are free from all sources of fallacy. On looking over my notes, I find, that seven of the seventeen experiments were performed before we obtained any evidence of digestion, and that the four experiments in which this was observed followed each other almost in succession; and this may so far explain how some experimenters may be more successful than others, when the number of their experiments is limited. It would appear that digestion is less impeded when the *vagi* are simply divided and their cut ends left in contact, than when they are separated from each other, or when a portion is removed. This interesting fact was elicited by the controversy between Dr Wilson Philip and Mr Broughton, * and appears to be so far confirmed by the experiments of Breschet, Milne Edwards, and Vavasseur. † In our experiments upon dogs, frequent vomiting invariably followed, for a certain time at least, the section of the nerves when food was taken into the stomach. This varied in degree in individual cases.—In Experiment XIII., (the first of the four experiments we have detailed above) the vomiting had almost entirely disappeared. In Experiment X. (the second experiment detailed above) the vomiting was very frequent; and we have seen this animal vomit, and again eat up the food vomited, several times within a few minutes. The death of this animal was, we believe, to be attributed to the frequency of the vomiting, for we had evidence that digestion was proceeding at the time of death, and the lungs were quite sound. The vomiting after section of the *vagi* is, we believe, generally occasioned by the presence of food in the stomach exciting the sensation of nausea, and not from any irritation of the upper ends of the cut nerves. For I have repeatedly observed dogs which made no efforts to vomit when the stomach was empty, seized with vomiting immediately or soon after food was taken into the stomach. So readily is vomiting excited in dogs by substances taken into the stomach after section of the *vagi*, that, in injecting various substances, such as prussic acid, laudanum, alcohol, &c., for purposes to which we shall afterwards have occasion to refer, I found it necessary to tie the *œsophagus* with a ligature, to prevent their rejection from the stomach. It has nevertheless been stated that large doses of emetics do not produce their usual effects when injected into the stomach after section of the *vagi*. ‡

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[*] Recherches Physiologiques et Chimiques pour servir à l'Histoire de la Digestion, p. 133–4. Paris, 1825.

[†] Oper. Cit., p. 135.

[‡] Dissertatio Inauguralis Physiologica sistens Experimenta quædam de Nervi Vagi in Digestionem Vi atque Potentia, p. 30. Gottingae, 1822.

[§] It is not expressly mentioned whether the œsophagus was at the same time divided or not, but we are led to infer that it was.

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[*] Oper. cit., p. 213–14–15.

[†] Oper. cit. Sir B. Brodie cut the *vagi* on the *œsophagus* in one experiment, and the digestion was arrested.

[‡] Opera Minora, Tom. i. p. 359–60. Experimenta 132–5–6. Haller states that he found the food in the stomach after section of the *vagi* putrid and converted into fæces.

[§] De Ligaturis Nervorum, Ludwig, Tom. ii. Scrip. Nerv. Min. Sel., p. 286-7. Experiments 2, 3, and 6.

[||] Propositions extraites d'un Essai sur la respiration, Paris, 1808.

[¶] Oper. cit., p. 354.

[**] Oper. cit. Dupuy believed that animals appeared to die after section of the *vagi* from suspension of digestion. Müller (Physiology, p. 358) represents Dupuy's opinion on this question as very different from this; but we have only to refer to p. 365 of the Journal containing the memoir of Dupuy, in proof of the correctness of what we have stated above.

[††] Oper. cit. p. 214.

[‡‡] Dissertatio Experimenta quædam de Ciborum Concoctione Complectens, Edin. 1818.

[§§] Oper. cit.

[||||] Quarterly Journal of Science, &c. p. 40. Vol. xi.

Effects of Lesion of the Vagi upon the Sensations of Hunger and Satiety.—Though these sensations are referred to the stomach as their seat, yet it is evident from well-established facts, that the stomach is merely the situation of the impressions made on the expanded extremities of the nerves by which these sensations are excited, and that the sensations themselves are actually connected with the encephalic portion of the nervous system. Are those impressions which excite the sensations of hunger and satiety made upon the expanded gastric filaments of the *vagi*, or upon the filaments of the ganglionic system, or upon both? If they depend entirely upon impressions transmitted to the *medulla oblongata* through the *vagi*, it is obvious, that, after section of the *vagi* and recurrents in the neck, these sensations could no longer be felt; for it is one of the conditions absolutely necessary for the excitation of any particular sensation, that there be integrity of the nerve upon which it is dependent, throughout its whole course between the organ upon which the impression was made, and the central organs of the nervous system. If the sensations of hunger and satiety depend upon impressions made upon the gastric branches of the *vagi*, it is obvious that, when these nerves are divided in the neck, the stomach may be in the most proper condition for receiving impressions, and the *medulla oblongata*, from which the nerve arises, may be in a fit state for the excitation of the sensation; yet it would be all to no purpose as long as the communication through the nerves between these two organs is interrupted. Brachet relates two experiments * to prove that the sensations of hunger and satiety are arrested by section of the *vagi*. From these he believes it is apparent that an animal, after section of the *vagi*, only eats to gratify the sense of taste, and that it continues to eat until the œsophagus and stomach are so much distended that it can swallow no more. The numerous experiments we have made do not by any means confirm these conclusions of Brachet. Though rabbits generally refuse to eat after section of the *vagi*, unless the food be placed under their nose, as in the experiments upon a dog and two Guinea pigs related by Brachet, yet I have seen one of these animals, immediately after the operation, make instantly for food, when this was thrown down in a corner of the room several feet distant from where it was at the time. Few of the dogs we operated on took food for the first twenty-four hours; and after all the coaxing we could practise, this was always in sparing quantities. Of the four dogs which lived beyond the fifth day, three cocked their ears, and looked out for food when we entered the room after they had fasted for several hours; and the quantity of food they took was variable at different times. In fact, we could not observe anything which would lead us to believe that they had lost the sensations of hunger and satiety. Of course it is quite possible that both these sensations might have been impaired by this operation; and it must be exceedingly difficult to ascertain how much an animal, in partaking of food, may be influenced by gratification of the sense of taste, and how much by the sensation of hunger. The mere retention of food in the œsophagus (the only proof adduced by Brachet) is no sufficient evidence that the food accumulates there from the over distension of the stomach caused by the loss of the sense of satiety; for in the rabbit, horse, sheep, and also occasionally in the dog, * this may arise from paralysis of the œsophagus.

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* Quarterly Journal of Science, &c. Vol. xi. p. 320, and Vol. xii. p. 17.

† Arch. Gen. de Med. Tome ii. p. 483.

‡ Brachet, Oper. Cit. p. 187.

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* Oper. cit. Experiment 52–3.

We believe, then, that no sufficient evidence has been advanced to prove that section of the *vagi* annihilates the sensation of hunger and satiety; while, on the contrary, there are strong grounds for believing in their occasional continuance. And in all experiments of this kind, we must carefully separate the accidental from the necessary consequences of the injury of an organ. There are certain peculiarities in the excitation of the sensation of hunger, which are worthy of being kept in remembrance in judging of the effects of lesion of the *vagi* upon this sensation. If the sensation of hunger depend upon certain impressions being made upon the expanded filaments of the gastric nerves, we are naturally led to inquire into the causes of those impressions. In the case of the eye, for example, we find that, in the healthy state of the organ, we can always observe some excitation from the rays of light or other causes, by which the impressions are produced upon the optic nerve, which give rise to the sensation of sight. In the same manner, in the healthy state of the ear or the other external senses, we are familiar with the excitants by which the impressions made on the nerves belonging to those organs of sense, are usually produced. Can we then also point out any physical or chemical change acting upon the expanded filaments of the gastric nerves, which may be supposed to produce the impressions which excite the sensation of hunger? Many physical and chemical explanations of the origin of the sensation of hunger have doubtless been propounded, but they are all most unsatisfactory. It has been, as Magendie observes, by turns attributed to the providence of the vital principle, to friction of the sides of the stomach against each other, to the dragging of the liver upon the diaphragm, to the action of the bile upon the stomach, to acrimony and acidity of the gastric juice, to fatigue of the contracted fibres of the stomach, to compression of the nerves of this viscus, &c. It would be mere waste of time to enter into any details to prove that each and all of the causes here adduced as the excitant of this sensation can have no such effect. We shall merely state, that some of these supposed causes cannot be present to excite this sensation; for example, there is no bile in the stomach in the natural state of that organ; and it is perfectly ascertained, more especially by the late researches of Dr Beaumont, that no gastric juice is secreted, or at least contained in the stomach, during the empty condition of the organ. This sensation, then, is independent, as far as we know, of any physical condition of the stomach itself, or of the presence of any substance in its interior, but arises from certain organic changes in the constitution generally, connected with the necessity for additional supplies of fresh materials from without; but in what particular manner these conditions affect the expanded extremities of the gastric nerves so as to excite the sensation of hunger, is at present a perfect mystery. It may be supposed that, since various substances taken into the stomach allay the sensation of hunger, though they cannot pass into the body to form a part of the nutritious juices,—and since various physical methods have been also found successful in producing the same effect, which cannot alter the conditions of the circulating fluid,—we have sufficient proof that this sensation is entirely connected with some particular state of the filaments of the gastric nerves, and not upon causes developed in the constitution, as we have here supposed. It is, for example, well known, that travellers find relief from the pangs of hunger by swallowing pebbles, or by compressing the abdominal organs by a tight girdle; and that some of the South American tribes swallow balls made of a kind of aluminous clay, when pressed by hunger. Even in an ordinary meal we are perfectly convinced that the sensation of hunger is completely allayed, and *that*, likewise, to the production of satiety, before the food has been in the slightest degree acted upon by the gastric juice. If this were not the case, this feeling of satiety would come too late to admonish us that enough has been taken into the stomach. All these, however, only succeed for a limited time in allaying this sensation; and if fresh chyle be not added to the blood, it will return, and become gradually aggravated in spite of all other attempts to relieve it. That these different methods should succeed for a time does not prove that this sensation is not excited by some organic cause developed in the constitution; for it is perfectly possible that, when the sensation is not in any great intensity, the particular condition of the filaments of the gastric nerves, induced by the organic causes developed in the constitution, whatever these may be, should be removed for the time by the physical effects of the several methods employed for allaying hunger. And this is borne out by the fact, that those succeed for a limited time only. Besides, it is also proved that the reception of food into the stomach, when not followed by the chymification and subsequent chylification of the food, does not even succeed in allaying the sensation of hunger after a time; as is illustrated by the fact, that, in cases of disease of the pylorus, where assimilation does not take place to any great extent, the patient may feel very hungry with a full stomach. The importance of having this sensation allayed, when the wants of the system exciting it are not yet very urgent, by the physical circumstances which generally lead to the removal of those wants, even before the means for supplying them are actually furnished; and the advantage of the insufficiency of all these methods to remove this sensation, when the wants of the system become very importunate, until the means themselves are actually applied to their removal, need only to be mentioned to be fully perceived. For it is obvious, that, unless the sensation when not urgent was removed in the manner in which it is, it could no longer serve as a valuable guide in admonishing us to finish our repasts; while, on the other hand, when the supply of nutritious juices fairly begins to fail in the system, by refusing to be contented with any thing less than a fresh supply of these, it forces the individual to look out for more nutritious materials.

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* Vide an experiment of Baglivi, formerly referred to.

Effects of Lesion of the Vagi upon the Secretions poured out on the inner surface of the Stomach.—We have, when discussing the effects of lesion of the *vagi* upon digestion, already detailed sufficient facts to show, that removal of a portion of both *vagi* and recurrents does not always arrest the digestive process, and consequently does not necessarily prevent the secretion of the gastric juice. In two of our experiments we ascertained that the half-digested food permanently reddened litmus-paper; and we consider the presence of chyle in the lacteals and thoracic duct, as observed by Leuret and Lassaigne, and in three of our experiments, as furnishing decisive evidence of the secretion of gastric juice. In experiments by Dieckhof and Müller upon the effects of section of the *vagi* in geese, they found the fluid secreted in the stomach to be acid, but less so than in the sound animal. It is further stated, that they never found the acidity absent.[*] Mayer also found the chyme acid in rabbits after section of the *vagi*. Breschet, M. Edwards, and Vavasseur, Dr Holland,[†] and Brachet, maintain that in their experiments the gastric juice was secreted, as the food in the stomach was more or less altered;[‡] but many of these experiments are liable to certain objections, particularly where the animals were fed before the nerves were divided, for it is then impossible to decide how much gastric juice was secreted before the operation was completed. In the experiments of Breschet and Milne-Edwards, in which they employed mechanical irritation of the lower ends of the nerves, the digestion was too far advanced to be explained by any supposition of this kind; for they state that, in one experiment, the alimentary matter was in a great degree reduced to pulp, the lacteals were full of chyle, and the digestion was as far advanced as when they employed the galvanic pile.* The positive proofs which we have here adduced of the secretion of the gastric juice cannot be invalidated by the negative results obtained by Tiedemann and Gmelin,† in an experiment upon a dog where the substances vomited after section of the *vagi* were found to be alkaline. As we believe that we have furnished adequate proof that secretion of the gastric juice may occur even when no mechanical or chemical irritation is applied to the lower extremities of the nerves, we have not thought it necessary to examine the supposed efficacy of galvanism, in supplying the want of the usual nervous influence transmitted along these nerves. We shall merely remark, that though it appears from the experiments of Wilson Philip, Dr Clarke Abel,‡ and Breschet, Vavasseur, and Milne Edwards,§ that galvanism transmitted along the cut *vagi* nerves to the stomach very much facilitates the digestive process, yet we are led to believe, from the experiments of Brachet, that it sometimes fails to do so, and Müller asserts that, in several experiments performed by himself and Dr Dieckhof upon rabbits, the application of the galvanism in the mode recommended by Dr Wilson Philip had no effect in promoting digestion.||

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[*] Müller's Physiology, Vol. I. p. 594. Translated by Baly.

[†] Oper. cit Chapter x.

[‡] The experiments of Mr Broughton (Quarterly Journal of Science, &c. Vol. x. p. 292,) are inconclusive, as the nerves were simply divided and the cut ends left in contact.

Secretion of Mucus.—We have examined the stomachs of many animals at various periods after section of the *vagi,* and we have never seen any thing which would lead us to suppose that the usual mucous secretions are there arrested; on the other hand, we have every reason to believe that they were poured out in the usual quantity, and presented their usual physical properties. When the stomach was empty it was generally found contracted upon itself. The inner surface never presented the inflamed appearance described by Gendrin as the consequence of division of the *vagi.*¶ Sir B. Brodie relates four experiments, in which animals were poisoned with arsenic after section of the *vagi,* where the usual watery and mucous secretions did not take place from the inner surface of the stomach and intestines, though the mucous membrane was inflamed.** In three of these experiments the *vagi* were cut in the neck, and, in the fourth, they were divided on the cardiac orifice of the stomach, to avoid the effects of the operation upon the breathing. In three, the animals were poisoned by introducing ten grains of white oxide of arsenic into a wound in the thigh; in the fourth, arsenious acid was dissolved and injected into the stomach. Sir B. thinks that the obvious conclusion from these experiments is, "that this secretion is pre-

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* Oper. cit. Tome vii. p. 195-6.

† Oper. cit.

‡ Medical and Physical Journal, Vol. xliii. p. 385.

§ Oper. cit. Tome ii.

|| Physiology, &c. p. 550.

¶ Histoire Anatomique des Inflammations, Tome i. p. 584. 1826.

** Phil. Transact. 1812, p. 102.

vented in consequence of the nervous influence having been interrupted by the division of the nerves of the eighth pair."* As it appeared improbable that section of the *vagi* should arrest the secretion of the mucus of the stomach and intestines, when it fails to suspend the secretion of the gastric juice, we were anxious to satisfy ourselves on this point. With this view we performed five comparative experiments, selecting two dogs as nearly as possible of equal size for each experiment. In four of these experiments the white oxide of arsenic was introduced into a wound in the thigh, in the quantity employed by Sir B. Brodie, and in the fifth it was introduced into the stomach. In each experiment the nerves were left untouched in one of the animals, while in the other both were divided and a portion removed, before the arsenic was introduced into the system.

Exp. XXXVIII.—Two powerful and large dogs. The *vagi* and one of the recurrents were divided in one of the animals, and ten grains of white oxide of arsenic moistened with water were introduced into a long incision in the thigh, and the wound was then stitched up. The same quantity of arsenic was at the same time introduced into the thigh of the other dog, the *vagi* being left entire. The dog in which the *vagi* had been divided appeared to be little affected three hours after the inoculation of the poison. Six hours after, it had vomiting of frothy mucus, and was very dull. Eleven hours after, it was found lying extended in a state of stupor, with frequent motions of the limbs. It was found dead next morning. The dog in which the *vagi* were left entire was very slightly affected after six hours. After eleven hours it was very dull, but was observant of surrounding objects. Twenty-three hours after, it was very feeble and languid, but still lay with the head erect. Other ten grains of arsenic were introduced into an incision made into the opposite thigh twenty-eight hours after the first inoculation of the arsenic. It was still alive, but very feeble and stupid. It was not seen for twelve hours after this time, when it was found dead and cold.

Sectiones. Dog in which the vagi had been cut.—A quantity of greenish fluid flowed from the mouth, before we proceeded to open the chest. The stomach and a considerable portion of the small intestines were removed after being enclosed in ligatures, and then slit open. *Stomach.*—The stomach contained a considerable quantity of greenish fluid, similar to that which flowed from the mouth. The organ was somewhat distended, presented few *rugæ* on its inner surface, which was slightly tinged green. A thin layer of mucus could be wiped off in several parts. *Intestines.*—A thick layer of brownish mucus covered the whole of the inner surface of a great part of the small intestines. This mucus was in great abundance at the upper part, and at some places flowed from the intestine when held up, or even when one of the edges was somewhat raised. The mucous membrane itself was of a uniform red colour. The blood in the heart was coagulated.

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* Oper. cit. p. 105.

Dog in which the vagi were not divided. Stomach. This organ also contained a greenish fluid (apparently tinged with bile,) certainly not more abundant than in the other dog, but there were more flakes of mucus floating in it. Mucous surface corrugated and red in some parts. *Intestines.* A thick layer of yellowish mucus covered the inner surface of a great portion of the small intestines, and the quantity did not differ decidedly from that observed in the former dog, when the two were compared together. The mucous membrane, however, in this animal was decidedly redder and more thickened, and there were several patches of elevated glands of a bluish colour in the upper part of the small intestines, similar to those found in the ileum in the human species, but more rounded. Some decolorized clots of blood in the *venæ cavæ.*

Both animals had been fasting when the experiment was commenced, and were allowed neither solids nor fluids up to the time of death. The same precaution was adopted in the other four experiments.

Exp. XXXIX.—The previous experiment was repeated upon two small dogs. Three and a-half hours after the introduction of the poison, neither animal seemed to be much affected, but the one in which the nerves had been cut was vomiting a good deal of frothy white mucus, and eight hours after the commencement of the experiment it was found dead. The animal in which the nerves were left untouched was standing at the end of eight hours, but evidently dull, and was vomiting frothy mucus. In four hours more (or twelve hours from the commencement of the experiment) it was found dead.

Sectiones. Dog in which the vagi had been cut. Stomach.—The stomach was contracted, and contained some yellowish fluid with flakes of mucus floating in it; the mucous surface was corrugated and slightly reddened, and there was very little mucus adhering to it; no softening of the mucous membrane. *Intestines.* A considerable portion of the small intestines was removed and slit open. A large quantity of thick yellow mucus lay on the inner surface, and could be partly made to flow out on holding up the edge of the intestine. The mucous membrane itself, particularly at the upper part, was thickened, reddened, and softened, and presented several elevated patches of glands. Much grumous blood in heart and large veins.

Dog in which the vagi were not divided. Stomach.—This organ was contracted, contained some greenish fluid, and its mucous membrane was more red than in the other dog. *Intestines.* —A very considerable quantity of reddish-brown thick mucus lined the inner surface of the upper portion of the small intestines, certainly presenting no decided difference in quantity from that in the former dog. The mucous membrane was reddened and softened, and the glands were elevated, of a blue colour, and surrounded by a red margin.

The blood was thick and grumous.

Exp. XL.—Two stout dogs. The previous experiment was repeated. The dog whose *vagi* had been cut, was found, twenty-three hours after the introduction of the arsenic, in a state of deep coma, and the respirations few and heaving. When visited the previous evening, it had vomiting and diarrhœa. It died twenty-five hours after the commencement of the experiment. The other animal was dull and languid thirteen hours after the inoculation of the poison, but had not vomited. Twenty-four hours after, it was more dull and languid, had some diarrhœa, but no vomiting. Other ten grains of arsenic were introduced into the same thigh, and near the former incision. At the forty-eighth hour was very languid and feeble; diarrhœa continued, but without vomiting. Other ten grains of arsenic were introduced into the opposite thigh. At the fifty-second hour it was still without coma, though very feeble and languid. Its death was hastened by a blow on the head.

Sectiones. Dog in which the vagi had been divided.—Stomach. The stomach contained a small quantity of a brownish fluid; the mucous surface was corrugated, and of a dull-red colour throughout; only a small quantity of mucus in the stomach. *Intestines.* The whole extent of the small intestines contained much brown fluid mucus, which ran out on slitting them open. Numerous small red patches presented themselves on the mucous surface, particularly at the upper part; while in the intervals between these the membrane was of its natural appearance. Blood thick and grumous, with small soft coagula.

Other dog. Stomach. About twice the quantity of fluid found in the stomach of the former dog was contained in this. The mucous surface was thrown into numerous *rugæ*, was of a deep-red colour, and presented many dark elevated spots evidently from effused blood; some of these were of a greenish colour, somewhat like sloughs. *Intestines.*—The mucus found in the small intestines was not decidedly greater in quantity than in the former, but the mucous membrane was everywhere of a very bright red colour, evidently thickened and inflamed, though the glands were not apparent. The stomach and intestines of a dog killed twenty-four hours after division of the *vagi* were compared with those of the two animals employed in the experiments we have just detailed, and presented a marked difference in the quantity and physical appearances of the mucus found in the intestines.

We need not detail the other two comparative experiments, as they did not differ in any essential point from those we have related. In Experiment XLI. two drachms of white oxide of arsenic were introduced into the stomach, and the œsophagus tied. The dog in which the *vagi* were divided died in convulsions seven hours and thirty minutes after the commencement of the experiment. The dog in which the *vagi* were left entire was seen alive, though feeble, at the end of eight hours, and was found dead two hours after this. The stomach was more inflamed in the latter than in the former, but there was evidently a greater quantity of effusion in the interior of the small intestines in the former than in the latter, which in both consisted of a whey-coloured fluid with flakes of mucus floating in it. In Experiment XLII. the arsenic was introduced into a wound in the thigh. The dog in which the *vagi* were cut was seen alive at the end of thirteen hours, and was found dead and cold at the end of twenty-four hours. At this time the dog in which the *vagi* were left entire was still alive, but was evidently very feeble, and had diarrhœa. The stomach in both of these animals was of a red colour, and contained some fluid. Much thick brown mucus flowed from the whole tract of the small intestines in the animal in which the *vagi* were not divided, and a thin greyish mucus from the other. The mucous surface of the small intestines was inflamed, especially in the upper part, but considerably more so in the animal in which the *vagi* were left entire, than in the other. In three of these comparative experiments the stomach and intestines were slit open in presence of Dr Alison. The stomach and intestines of the two animals the subject of each experiment, were always examined at the same time, and in this manner were accurately compared with each other. Though the quantity of watery and mucous effusions from the inner surface of the digestive canal was apparently nearly the same in the two animals of the same experiment, and, in fact, presented no very marked difference in any of the ten animals killed in this manner, yet in all the experiments except in Experiment XXXIX. the mucous membrane of the stomach and intestines was more inflamed in the animals in which the *vagi* were not divided, than in those in which they had been previously cut. This, we believe, can be sufficiently accounted for by the circumstance, that the animals in which the *vagi* were divided died generally several hours later than those in which they were left entire.

We cannot pretend to account for the discrepancy between the results obtained in the experiments detailed above and those related by Sir B. Brodie. It is possible, however, that Sir B. had not taken the precaution of securing a free ingress of air into the lungs in the three experiments in which the nerves were cut in the neck, for it is stated that in the first experiment, the breathing of the animal was labouring, and it died after three hours and a-half. In the third experiment the animal only lived three hours. In our experiments one animal in which the *vagi* had been divided died after the eighth hour, and the others lived considerably longer. It must, however, be mentioned, that in the second experiment related by Sir B. the animal lived nine hours. Even in this case the death might have been hastened by the effects of the section of the nerves upon the muscular movements of the larynx. Whatever be the cause of this discrepancy, it is obvious that the negative evidence obtained by Sir B. Brodie cannot affect the value of the positive experiments we have detailed; and it necessarily follows that the conclusion deduced from it by Sir B. is not universally or even generally applicable; whilst we believe we are on the other hand justified in concluding, that lesion of the *vagi* does not necessarily prevent the usual watery and mucous secretions from the inner surface of the stomach and intestines. From all the experiments we have performed upon the effects of lesion of the *vagi* upon the different secretions poured out upon the inner surfaces of the stomach, we have arrived at the conclusion, that, if the agency of the nervous influence be at all necessary for the elimination of these from the blood, this may be affected by the ganglionic system after a portion of both *vagi* has been removed in the neck. We have also, we believe, adduced sufficient evidence to set aside all the arguments which have been drawn from the effects of lesion of the *vagi* nerves upon the functions of the stomach, by those who maintain that secretion depends upon the agency of nervous influence. Though the experiments we have stated cannot, of course, be adduced in proof of the opinion, that secretion is not necessarily dependent upon nervous influence, since other nervous filaments are distributed in the stomach,—yet they nevertheless completely invalidate some of the arguments which have been strongly insisted on by the supporters of the opposite doctrine.

The varied experiments upon the *vagi* ought to be viewed in two different aspects; for while they show that integrity of those nerves is not a condition absolutely necessary for the performance of secretion in the stomach, they yet prove that the secretions usually poured into the interior of that organ may be modified, in a most important manner, by causes acting through these nerves. We have here two perfectly separate and distinct propositions, which have sometimes not been clearly distinguished from each other. The difference between them may be illustrated in a very simple manner. The movements of a horse are independent of the rider on his back,—in other words, the rider does not furnish

the conditions necessary for the movements of the horse,–
every one knows how much these movements may be influence
the hand and heel of the rider. Though it would be out of
to enter here at any length upon the theory of secretion, ye
cannot avoid remarking, that the important influence which l
of the *vagi* exerts upon the secretion of the gastric juice, must
vent us from attaching much importance to the negative ex
ments of Naveau,[*] Krimer,[†] Brachet,[‡] and Müller,[§] upon
effects of lesion of the renal plexus of nerves upon the secreti
urine from the kidney. When we extend our investigation
the manner in which the function of secretion is performed t
whole range of the vegetable and animal kingdoms,[||] we ca
think, have little difficulty in arriving at the conclusion, that
nerves exercise over secretion " not an uniform and essential,
an occasional and controlling influence."[¶]

Effects of Lesion of the Vagi upon the rapidity of Absorp
from the inner surface of the Stomach.—It has been st
by Dupuy[**] and Brachet,[††] that after the *vagi* are divi
the most active poisons, introduced in much larger doses t
usual, produce their effects more slowly, and with less ene
On the other hand, Müller asserts, that in thirty experiment
Mammalia, which M. Wernscheidt performed under his di
tions, " not the least difference could be perceived in the ac
of narcotic poisons introduced into the stomach, whether the
vus vagus had been divided on both sides or not, provided
animals were of the same species and size."[‡‡] I have made se
ral comparative experiments,—the details of which it will be
necessary to give,—in which I injected prussic acid, alcohol,
laudanum into the stomach, taking care, of course, that the
animals selected for each comparative experiment should b
nearly as possible of the same strength, that equal quantitie
the same poisons should be injected into the stomachs of b
and that the œsophagus should be tied by a ligature to prevent th
jection of the contents of the stomach by vomiting. In these e
riments I could perceive no decided difference between the rapi
of the action of the poison in the animals in which the *vagi*
been cut, and those in which they were left entire. On the o
hand, I have seen animals in which the *vagi* were uninjured
cover from doses of narcotic poisons, which proved fatal to o

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[*] Dissertatio Inauguralis sistens Experimenta quædam circa Urinæ Secretio
p. 16, 17, 18. Halae, 1818.

[†] Vide Lund's Physiologische Resultate der vivisectionen neurer Zeit. S. 20

[‡] Oper. cit. Chapitre v.

[§] Physiology translated by Baly, Vol. i. p.

[||] Carpenter's Principles of General and Comparative Physiology, Chap. xi.

[¶] Alison in Vol. ix. of Quarterly Journal of Science, &c. p. 124.

[**] Oper. cit. p. 366.

[††] Oper. cit. p. 186.

[‡‡] Physiology, p. 246.

animals of equal or even superior strength, in which the nerves had been previously divided. And this is nothing more than what we should expect, when we remember that the effects of the diminished frequency of the respiration are conjoined to those of the narcotic poison.

Cause of Death after Section of the Vagi.—From what we have already said it is obvious, that though the animals in which the *vagi* have been divided generally die from morbid changes in the lungs, yet they occcasionally die from inanition produced by the derangement of the digestive organs. Experiment X. is an example of the latter mode of death. We are of course here supposing that means have been taken to insure the access of a sufficient quantity of air into the lungs; otherwise the animal may die, suffocated from the arrestment of the movements of the muscles attached to the arytenoid cartilages.

From the numerous experiments we have made upon the *nervi vagi* and their branches, we have arrived at the following conclusions.

Pharyngeal Branches.—The pharyngeal branches of the *vagi* are entirely, or almost entirely, motor, and move the muscles of the pharynx and soft palate.

Laryngeal Branches.—The superior laryngeal are almost entirely nerves of sensation, supplying the mucous surface of the larynx, and part of the pharynx, with sensitive filaments. The few motor filaments which they contain are distributed in and move the crico-thyroid muscles. The inferior laryngeal nerves are ramified in and regulate the movements of all the muscles attached to the arytenoid cartilages, viz. the *crico-arytenoidei postici* and *laterales*, the *thyro-arytenoidei*, and *arytenoidei*. The inferior laryngeals also furnish the sensitive filaments to the upper part of the *trachea*, a few to the mucous surface of the pharynx, and still fewer to the larynx. When any irritation is applied to the mucous membrane of the larynx in the healthy state, this does not excite the contraction of the muscles which approximate the arytenoid cartilages, by acting directly upon them through the mucous membrane; but this contraction takes place indirectly, and by a reflex action, in the performance of which the superior laryngeals act as the sensitive or afferent nerves, and the inferior laryngeals as the motor or efferent nerves. It is also probable that those branches of the inferior laryngeal distributed in the muscular fibres of the *trachea* are motor.

Œsophageal Branches.—The œsophageal filaments of the *vagus* are partly afferent and partly efferent nerves. In some animals, as in the rabbit, the section of the *vagi* in the neck is followed by arrestment of the passage of the food along the œsophagus not from destroying the contractility of the muscular fibres of the œsophagus, but by breaking the continuity of the nervous circle necessary for the accomplishment of all reflex actions.

Cardiac Branches.—Though the movements of the heart may be materially influenced by causes acting through the *vagus*, yet mental emotions and injuries of the central organs of the nervous system affect the heart's action, after the *vagi* and recurrents are divided in the neck.

Pulmonary Branches.—The pulmonary branches of the *vagus* furnish the principal channel by which those impressions made at the lungs, capable of exciting the respiratory muscular movements, are transmitted to the *medulla oblongata.* It also appears that they transmit the impressions which excite the sensations of coughing, &c.; but we are inclined to believe that this may likewise be effected to a certain extent by the ganglionic system. It is probable also that the pulmonary branches contain motor filaments, though we have not been able to obtain any decided evidence of this.

Gastric Branches.—Lesion of the gastric branches does not necessarily arrest the muscular movements of the stomach, or the usual secretions poured out from its inner surface; though each and all of these, but particularly the secretions, may be modified to a very important extent by causes acting through the nervous system.

ART. II.—*Observations on the Physiological Effects of various Agents introduced into the Circulation, as indicated by the Haemadynamometer.* By Mr JAMES BLAKE, London.

THE subject which has given rise to these researches is, "The action of various substances on the animal economy when introduced into the veins, more particularly as regards the influence they exert in modifying the circulation."

My attention was first directed to this subject by a remark of Professor Sharpey, in his Physiological Lectures, on the extensive field of inquiry which was opened to us in this branch of physiology, by the invention of the haemadynamometer. As it is by the aid of this instrument that the results at which I have arrived have been obtained, it will perhaps render the description of the experiments more intelligible, if, before proceeding, I give a short explanation of it.

I. *Description of the Haemadynamometer.*—It consists of a glass tube, so bent as to form two vertical branches and a horizontal one. To the horizontal branch is adapted a brass cap, which serves to connect it with a tube, which is inserted into the artery. The glass tube is fixed to a graduated scale. When used mercury is poured into it, until it attains in the vertical arms, a height corresponding to the zero of the scale. It is then connected with the tube inserted into the artery, and on turning a stop-cock, with which the tube is provided, the blood enters the instrument, and the pressure in the arteries is instantly indicated, by the difference in the height of the surface of the mercury in the vertical branches. The great advantages offered by this instrument are; that it enables us to detect any change in the action of the heart the instant it takes place, and also affords us a means of substituting the accurate expression of so many inches of mercury for the undefined terms, more or less strong.

Cause of Pressure in the Arteries.—Before entering into a detail of the experiments I have to bring forward, I shall offer a few remarks on the cause of the pressure in the arterial system, and how this pressure can be modified. As we are now treating a subject which is entirely under the dominion of physical laws, I shall consider the arterial system as composed of a set of elastic tubes, into which a constant supply of fluid is sent by the action of the heart, and from which a corresponding quantity is discharged by a number of small orifices, the capillaries. It is evident, that, in order that there should be any amount of pressure sustained by the parietes of these tubes, the quantity of fluid sent into them must be more than can be freely discharged by the discharging orifices, and that, in fact, the amount of pressure which they sustain will be in proportion to the quantity sent in, above that which can be freely discharged. It also follows, that any variation in the pressure which may be supported by the parietes of these tubes, may depend either on the quantity of fluid sent into them in a given time being increased or diminished, or an alteration in the discharging orifices. From these remarks, it is evident that the pressure in the arterial system, as shown by the haemadynamometer, is nothing but an expression of the force required to cause a certain quantity of blood to pass through the capillaries in a given time.

I have been thus particular in describing the manner in which the pressure in the arteries may be modified, as our inquiries as to the manner in which certain substances act will be much facilitated by keeping these simple hydraulical laws in view.

The changes in the physical properties of the blood must also be an important element in modifying its passage through the capillaries, but it is one which the present limited state of our knowledge, as regards these changes, does not enable us fully to estimate.

Effect of Respiration on the Heart.—With these remarks I shall close the preliminary observations which I have to offer, and shall now proceed to a detail of the experiments which I have performed, in order to elucidate the manner in which various substances act, when taken into the circulation. Many of these, although speedily causing death, seem to exert no immediate effect on the heart,—this organ remaining apparently unaffected, until its supply of arterial blood is cut off, owing to the respiratory movements being stopped. I shall, therefore, commence by relating an experiment which was performed to ascertain the effects of respiration on the action of the heart.

The animal on which the experiment was performed was a dog. A tube was introduced into the trachea, and an opening made in the parietes of the thorax, so as to prevent air from entering the lungs by the action of the respiratory muscles. Artificial respiration was had recourse to. The pressure in the arterial system was taken by the haemadynamometer, (which, for the sake of brevity, I shall in future designate as the instrument,) being connected with a tube, which was inserted into the femoral artery. It was found to be equal to a column of mercury of six inches and a-half. All access of air to the lungs was now cut off; the pressure in the arterial system gradually diminished. In two minutes, the column of mercury supported was only three inches and a-half, the pulsations of the heart having become much slower. The lungs were now inflated with air; this was instantly followed by a quickened action of the heart, and an increase of the pressure in the arteries, which was indicated by the mercury in the instrument rising about 25 seconds after the injection of the air. Air was again prevented from entering the lungs during a space of three minutes and a-half. The column of mercury had now diminished to two and a-half inches, the heart pulsating very slowly. Air was now injected into the lungs with exactly the same effect as before. After three respirations had been performed, the column of mercury had again attained its former level of six inches and a-half. After the supply of air had been cut off for eight minutes and a half, the column of mercury had diminished to one inch; the heart was still observed pulsating. Air was again injected into the lungs, but this was not followed by any action on the heart, although it was observed pulsating for three and a-half minntes after this time.

Effects of Transfusion of Blood.—The effect of the transfusion of blood on the pressure in the arteries is a subject which has already been fully investigated by Magendie. He found that the injection of eighteen ounces of blood in the veins of a dog produced no increase of the pressure in the arterial system. But it appears, that the pressure in the arteries of the animal from which it had been abstracted, had become diminished; and I think it highly probable that its reinjection into the veins of this animal would have caused the pressure in the arteries to have been again increased. The detail of these experiments will be found in the third volume of his " Leçons sur les Phénomènes Physiques de la Vie."

Injection of Water.—The injection of water into the veins is a subject which has also been investigated by this able physiologist; but on this point the experiments which I have performed do not agree with those which he has related. He states that the injection of about a pint of warm water into the jugular vein was followed by the sudden diminution of the pressure in the arterial system. On repeating this experiment, I have, on two occasions, found that an opposite effect was produced, or that the introduction of a considerable quantity of water into the veins is followed by an increase in the pressure in the arteries. Two pints of water, at a temperature of 102° Fah., were gradually injected into the veins of a dog, and it was found that the column of mercury supported by the blood in the arteries was full an inch and a half higher than it was before the operation. This result receives support from the effects produced by the injection of warm water into the veins in cases of cholera, which are evidently not such as would indicate that the action of the heart is diminished. I think it probable that the animal operated on by Magendie was in a sinking state before the injection of the water, as it had already served for one or two experiments.

II. *Classification of Substances.*—I shall now proceed to investigate the action of those substances, which, when injected into the veins, exert a marked influence on the animal economy. For reasons, which will be obvious as we proceed, it will be advisable to divide these substances into three classes. The first class will comprehend those which by acting directly on the contractility of the heart, and which, when injected into the veins, produce death, by suddenly stopping the contractions of that organ. The second class will contain those substances, which, although causing death quite as rapidly or even more so than the former, yet leave the heart to all appearance unaffected, the action of the poison being directly on the nervous system. The third class will include those, which, though acting directly on the nervous system, yet agree in exerting a powerful influence in modifying the capillary circulation. In a fourth class I shall advert to the action of several substances which cannot be referred to either of the three foregoing heads. Imperfect as the first investigations in so comprehensive a subject must necessarily be, I think that these researches, in so far as they have been conducted, will enable me to localize the action of the greater number of the substances experimented on, so as to justify my placing them in one or the other of these classes.

1. *Substances acting on the contractility of the heart.*—

The substances which will come under the first class, and of which the action on the heart has been experimentally investigated are the following:—the nitrate, arsenite, and carbonate of potass, carbonate of soda, ammonia, iodide of arsenic, oxalic acid and galls.

Injection of Nitre.—A description of the manner in which nitrate of potass acts when injected into the veins will furnish us with a good example of the action of this class of substances; and by giving a detail of an experiment with this salt, I shall be enabled to dismiss the others, with a few remarks on their action, in so far as they may differ from it. The animal operated on was a dog. A tube was inserted into the jugular vein, by which to inject the poison; another tube was inserted into the femoral artery, by which to connect the arterial system with the instrument, in order to ascertain the pressure. Immediately before the injection of the poison, the column of mercury was found equal to six inches.

Action of Nitre.—A solution containing fifteen grains of nitrate of potass, dissolved in six drachms of water, was injected into the vein. Ten seconds after the injection, the column of mercury in the instrument began to sink, the pressure in the arteries rapidly diminishing, owing to the action of the heart being suddenly stopped. Thirty seconds after the injection of the salt, the column of mercury supported was only equal to an inch and a half. The animal continued struggling for some time after the heart had stopped beating; and the respiratory movements continued 20 seconds after the cessation of the circulation. The struggles were evidently not convulsive, and the animal appeared quite sensible until death took place, which happened about 45 seconds after the injection of the poison. The thorax was opened immediately after death; the heart was exposed within a minute and a half after the injection of the poison. The right auricle was observed to be still pulsating, but not the slightest contraction could be produced on any other part of the heart, even by the galvanic stimulus. The left side of the heart was opened, and found to contain a considerable quantity of blood of a bright scarlet colour. The right side of the heart contained a quantity of dark blood.

When a very weak solution of this salt is used, its immediate effect is to cause dyspnœa, the action of the heart being rendered irregular; the pulsations after a short time become quicker, and the pressure in the arterial system increased. A solution containing a grain of the salt produced these effects when injected into the veins, the height of the column of mercury being increased half an inch. Two grains, when injected, produced an increase of pressure in the arteries equal to two inches of mercury. Three grains were then injected into the veins of the same dog. In 25 seconds after the injection, the pulsations of the heart were suddenly stopped, and were not again renewed. The thorax was immediately opened, the state of the heart was exactly the same as in the last experiment; the right auricle still contracting, but all the other parts being insensible to stimuli.

Action of Arsenite of Potassa.—Arsenite of potass, when injected into the veins, much resembles nitre in its action on the heart. It does not appear, however, to exert so powerful an influence in destroying the irritability of this organ as nitre. The injection of a solution containing 15 grains of this salt was followed by dyspnœa in about 10 seconds; and the sudden cessation of the action of the heart, 20 seconds after its introduction into the veins. On opening the thorax the right auricle contracted on being irritated, but the left ventricle could not be made to contract, even when galvanism was applied. The left sides of the heart contained a considerable quantity of scarlet blood.

Action of Carbonate of Potass.—Carbonate of potass is analogous in its action to nitre, although it does not appear to be so deleterious an agent. A solution containing five grains, when injected into the veins, gave rise to the same phenomena as were produced by the injection of a grain of nitre. A solution containing twelve grains of this salt suddenly arrested the action of the heart in 15 seconds after its injection; a few slight and apparently partial pulsations again took place, but these soon ceased, and the animal was dead in about a minute and a half after the introduction of the poison. The necroscopic appearances were the same as those described in the last experiment, the heart retaining after death a slight degree of irritability. The blood in its cavities was firmly coagulated.

Action of Subcarbonate of Soda.—I have not myself investigated the action of subcarbonate of soda; but I find in an experiment recorded by Magendie, that its injection into the veins in a concentrated state was almost instantly fatal. He attributes the deleterious action of this substance, to the effect it has in preventing the coagulation of the blood, and thus causing effusion into the surrounding tissues; but I consider there can be but little doubt, that it was by affecting the irritability of the heart that it so suddenly caused death.

The action of subcarbonate of soda on the heart is a subject which has been experimentally investigated since writing the above paragraph; and the results of the experiments, certainly, do not bear out the explanation which has there been given of Magendie's experiment. In the experiments performed by me the heart retained in a marked degree its irritability, after death had been caused by the injection of a strong solution of subcarbonate of soda into the veins. Extensive ecchymosis had also taken place in the lungs; but at the same time the right side of the heart was not distended

with blood, nor was the left side in so empty a state as would
been the case, had death been caused by an arrest of the pulmo
circulation.

Action of Ammonia.—The action of the ammonia also
subject, which I have not experimentally investigated; but fro
case related, with a rare degree of professional candour, in
Medical Gazette for December 30, 1837, I think it is evid
that its introduction into the blood is followed by results
cisely analogous to those produced by the substances whose ac
we have been considering. It is there stated that the injectio
a solution of ammonia into a nævus was followed by the sud
death of the subject of the operation, and which, from the sy
toms described, I consider was owing to the action of the h
being stopped by some of the injection having entered the vein

The supposition now advanced as to the power of ammon
arresting the pulsations of the heart has since been confirmed
direct experiment. It was found that a drachm of a strong s
tion of ammonia, mixed with six drachms of water, stopped
action of the heart twenty-five seconds after its injection.
heart retained a slight degree of irritability after death.

Action of Iodide of Arsenic.—Iodide of arsenic, althoug
poisonous a substance, does not appear to exert so strong an ac
on the heart as might be expected. Solutions, containing
six grains of this substance, were twice injected into the jug
of a dog, without producing the slightest appreciable effect on
heart. On injecting a solution, containing fifteen grains of
substance, the action of the heart was immediately stopped. O
the right auricle retained its irritability after death.

Action of Oxalic Acid.—Oxalic acid, when introduced into
veins in small quantities, does not appear to exert any direct
tion on the heart. The injection of half a drachm into the jug
vein was followed by the sudden cessation of the action of the he
The blood in the left ventricle was scarlet.

Action of Infusion of Galls.—The effect of the injection
weak solution of galls was to cause the pulsations of the hear
become slower, the pressure in the arterial system being dimin
ed. Six drachms of a concentrated infusion caused the heart to
15 seconds after its injection. On opening the thorax the h
contracted on exposure to the air, the left ventricle contracting
parently more readily than the right. Dark blood was found
both sides of the heart, although respiration did not cease u
the action of the heart had been arrested for some seconds. I th
it probable that the presence of dark blood in the left side mi
be accounted for by the previous injection of a considerable qu
tity of the substance having altered the physical properties of
blood.

Remarks on the Action of Nitre, &c.—Before proceeding to the investigation of the mode of operation presented by those substances which act directly on the nervous system, I shall offer a few remarks on the experiments already related.

The first fact that strikes us on a review of these experiments is the sudden manner in which the action of the heart is stopped, and the almost total absence of irritability in that organ after death. In all the experiments also, we have the absence of those symptoms which are usually considered as indicating an affection of the nervous system, the time and manner in which death occurred corresponding to that which would result had excision of the heart been performed. I think these facts are sufficient to justify the arrangements I have made of these substances, as proving fatal by acting locally on the heart.

Having attempted thus far to localize the action of these substances, the next question which presents itself is, do they act by being applied to the internal surface of the heart, or is it by circulating over its parietes? There are many reasons which would lead us to conclude that it is the circulation of these substances, when mixed with the blood, over the parietes of the heart, that produces the affection of that organ. The principal of these are, the absence of any immediate effect on the heart when the substance is injected, although it is then applied in its most concentrated state to the lining membrane of the right side;—the great dyspnœa produced before the heart is affected, indicating that the substance is passing through the lungs;—and the right side retaining its irritability after death, although it was to this part of the heart that the substance was applied in its most concentrated state. These, I think, are sufficient reasons for concluding that it is through the medium of the blood circulating over the parietes of the heart that these sudden effects are produced.

But the most important consideration connected with these experiments, is the endeavour to ascertain on what common property possessed by these substances, their similarity of action depends.

There is one property, however unlike these may be in other respects, in which they all agree, and this is, in effecting considerable changes in the chemical composition of the blood.

The change of colour produced by the presence of the various salts in the blood, the marked action on organic substances by the iodides, the dark colour imparted by the acids, and the important combinations formed by tannin and gallic acid with the constitution of the blood, are sufficient proofs that we are employing substances which exert a considerable influence in modifying the composition of this fluid. The present state of our knowledge in organic chemistry, unfortunately, is such, that we are unable even to form an idea of the nature of the changes which take place in the blood, so that all speculation on this point must be useless. There is one part, however, with which that science furnishes us, which will account for the small quantity of some of these substances necessary to prove fatal. This is the formation of definite compounds between organic substances and inorganic bases, containing not more than two or three per cent. of the inorganic base, so that the presence of a few grains of a salt in the blood may give rise to an alteration in a considerable portion of that fluid.

Amongst the substances whose action we have been considering, are some which are daily taken into the stomach in considerable doses, and which are undoubtedly rapidly absorbed; and yet their action on the animal economy, when taken in this manner, is certainly not such as might be supposed to result from the absorption of substances, the presence of which in the blood in any quantity must be productive of such serious consequences.

It is an interesting fact, however, that the therapeutical action of these substances (nitre and the carbonates of potass and soda) is to increase the secretion of the kidnies, by which they are again rapidly eliminated from the system, thus affording an effectual means of preventing their accumulation in the blood to any amount.

2. *Substances acting on the Nervous System.*—I shall now proceed to investigate the action of those substances on the circulation, which appear to produce death by destroying the functions of the nervous system.

The substances the operation of which I have experimentally investigated in this class are strychnia, conia, and hydrocyanic acid. The manner in which these substances act will be illustrated by the following experiment.

Action of Strychnia.—A grain of strychnia, dissolved in a small quantity of acetic acid, was injected into the veins of a dog. The first effect of the poison was to produce general convulsions about 30 seconds after its injection, the action of the heart being at the same time rendered irregular; but I think this can be fully accounted for by the convulsive struggles of the animal. All external signs of life ceased about a minute and a-half after the injection of the poison; the heart still continued beating. A minute after the apparent death of the animal, the column of mercury in the instrument indicated that the pressure in the arterial system was about equal to what it was before the introduction of the poison. The pressure in the arteries gradually became diminished, as in the experiment in which the effect of respiration on the heart was ascertained. Five minutes after death, the column of mercury supported was only equal to one inch and a-half. No farther diminution was observed, owing to the blood in the tubes having coagulated. The thorax was not opened for a quarter of an hour after death. The right sides of the heart then contracted on being exposed to the air. They contained a considerable quantity of dark-coloured blood. The left ventricle contained a small quantity of blood, which was as dark as that in the right.

Strychnia when introduced into the circulation in so small a quantity as not to produce any general symptoms, does not appear to exert the least influence on the heart.

Action of Hydrocyanic Acid.—Hydrocyanic acid when injected into the veins, although analogous to strychnine in its sudden action on the nervous system, yet appears also to exert a decided effect on the heart, not apparently by destroying in any degree its property of contracting, but by rendering the contractions irregular, and causing considerable variations in the pressure in the arterial system.

Five drops of hydrocyanic acid in six drachms of water were injected into the jugular vein of a dog, the instrument being connected with the femoral artery. In fifteen seconds the action of the heart became irregular. The column of mercury, which was before equal to about six inches and a-half, now oscillated between six and twelve inches, the greatest elevation being attained after two or three quickened pulsations. These great variations in pressure were in part due to the respiration being considerably affected, and to the struggles of the animal. But this cause certainly is not sufficient entirely to account for it. Forty-five seconds after the injection of the poison all respiratory movements ceased, and in a few seconds the animal appeared quite dead. The action of the heart, however, still continued. Immediately after the death of the animal, the column of mercury indicating the pressure in the arteries was about eight inches, or fully an inch higher than it was before the injection of the poison. The action of the heart became more regular a short time after death, the pressure in the arterial system gradually diminishing. Four minutes after death the column of mercury was only one inch and eight-tenths, the pulsations being still 100 in a minute. The thorax was opened about seven minutes after the apparent death of the animal. The right side was found to be still contracting occasionally. The left ventricle did not contract on the application of mechanical stimuli. Both sides of the heart were distended with dark blood, which was firmly coagulated.

The injection of half a drachm of hydrocyanic acid into the veins was followed by analogous results as regards the heart; in this instance, however, only ten seconds elapsed before it was affected, and the animal was killed more quickly.

Action of Conia.—The action of conia when injected into the veins is very analogous to that of hydrocyanic acid, except that the action of the heart was rendered more regular by its injection.

A single drop introduced into the veins began to produ
effects in about thirty seconds. These were first manifeste
the respiration being stopped, and by the occurrence of general
vulsions. The animal was apparently dead in about a minute.
heart still continued pulsating, and was ultimately stopped b
phyxia. On opening the thorax and pericardium the stimulus o
air was sufficient to produce general contractions of the heart.
sides were distended with blood, which was dark and coagula

Remarks on Strychnia, &c.—The action of these substa
strychnia, hydrocyanic acid, and conia, affords a striking con
with that of those contained in the former class.

In the one instance the action of the heart is suddenly a
ed, the functions of the nervous system being destroyed on
count of the circulation being stopped; in the other class of
stances we find those, whose action is first on the nervous sys
the heart remaining comparatively unaffected, carrying on the
culation as if independent of the inanimate mass which surro
it.

3. *Action of the third class of Substances.*—I shall now pro
to consider the action of the third class of substances, or those w
appear to exert a decided effect in modifying the capillary ci
lation. The property here taken as a ground of classificati
certainly not of such primary importance as that which has se
for the formation of the other classes, which are distinguishe
the manner in which the substances placed in them produce de

But in the class now under consideration it is not this prop
which has been taken as the ground of arrangement, for the
apparently prove fatal by acting on the nervous centres; but
find they all agree in exerting a considerable influence in mo
ing the capillary circulation, even when not given in any suffic
quantity to produce any marked effect on the nervous syste
have taken this quality as that which serves for their classificat

Action of Tobacco.—The first of these substances whi
shall consider is tobacco. The pressure in the arterial sys
before the injection of the poison, was equal to a column of
cury of six inches and a-half. An infusion from about ten g
of tobacco was injected into the jugular vein; in about 10 sec
the respiration was affected, it becoming very laborious and blow
Twenty seconds after the injection of the poison the action o
heart suddenly stopped, and in a few seconds the column of
cury had diminished to two inches. After the heart had sto
for a few seconds it again began pulsating, the pressure in th
terial system becoming considerably increased, and being irr
lar, the column of mercury oscillating between six and twelve inc
the oscillations often amounting to four or five inches at each
sation. In three or four minutes the pressure was again redu

the oscillations not being so great. The introduction of another injection of the same strength was attended with exactly analogous results. On injecting an infusion from two scruples of tobacco the heart was stopped in about 20 seconds, and remained arrested for 30 seconds; the column of mercury had diminished to one inch; but, on the heart again commencing to pulsate, the pressure in the arterial system suddenly became increased, and in the course of four or five pulsations was equal to a column of mercury of from ten to twelve inches. The mercury oscillated for some time between seven and twelve inches; it gradually, however, became more regular, and sank to about six inches and a-half. The animal did not appear much affected by the tobacco; the respiration was rather deep, but there were no convulsive movements or loss of consciousness.

An infusion from three drachms of tobacco injected into the veins of another dog was followed, as before, by the cessation of the action of the heart for some seconds; it then again commenced beating, and the pressure in the arterial system was much increased. The animal died about two minutes after the introduction of the poison much convulsed. The heart continued beating some time after death, and was finally stopped by asphyxia.

Action of Euphorbium.—Euphorbium when injected into the veins appears to act in a manner analogous to tobacco.

A solution when injected caused the heart to stop in about 20 seconds. The pressure in the arterial system immediately before its injection was equal to about four inches of mercury, but it suddenly became diminished, the column of mercury supported being only an inch and a-half. After the action of the heart had been suspended for 30 seconds it again commenced, and in ten or twelve pulsations the pressure in the arterial system had become so considerably increased, that the column of mercury after attained the height of fifteen inches, the parietes of the arteries thus being submitted to a pressure nearly four times as great as they sustained before the injection of the poison. The oscillations in the column of mercury were very considerable, often amounting to four or five inches at a single pulsation.

All external signs of life ceased about two minutes after the injection of the poison, but the action of the heart still continued. Artificial respiration was had recourse to three minutes after all signs of life had ceased, the pressure in the arteries being still equal to a column of mercury of nine inches. No immediate effect was produced by the inflation of the lungs. The pressure in the arteries gradually sank.

Ten minutes after the apparent death of the animal a most curious phenomenon presented itself. The column of mercury had by this time become diminished to two inches and a half, when in a few pulsations it again attained the height of eight inches, although not the slightest sign of life had been evinced for full ten minutes. Slight convulsive movements took place on this increase of pressure in the arteries, and continued for a few seconds. Artificial respiration was still continued, the pressure again gradually becoming diminished. At twenty-eight minutes after the apparent death of the animal the column of mercury was only about one inch and a half. The same extraordinary increase in the pressure again took place, the column of mercury in the course of a few pulsations attaining the height of four inches. This was not accompanied by the slightest perceptible movement of the animal, eighteen minutes having now elapsed since any had been observed. The motion of the heart was finally stopped by injecting a solution of euphorbium into the veins, after artificial respiration had been kept up for thirty-six minutes. After death extensive ecchymosis was found in all the viscera, particularly on the internal surface of the heart, and in the lungs.

This, I think, must have been the result of the great pressure, to which the parietes of the blood-vessels must have been subjected.

Action of Digitalis.—Digitalis agrees with tobacco and euphorbium in modifying the capillary circulation, but it appears also to exert a decided action on the heart, by rendering its pulsations much slower. On injecting an infusion from about a drachm of the leaves, the respiration was affected in five seconds; and ten seconds after the injection of the substance into the veins, the heart was affected, its pulsations being rendered so slow, that, in five seconds, the column of mercury indicating the pressure in the arterial system had diminished from five to two inches. After a few seconds the pressure in the arterial system again became increased, the column of mercury being as high as eight inches, the pulsations of the heart again becoming quickened. When an infusion from three drachms of the leaves was introduced into the jugular vein, the action of the heart was arrested five seconds after the injection of the poison, and no pulsation was indicated after this. The pressure in the arterial system diminished very slowly. One minute after the heart had stopped, it was still equal to two inches and a-half of mercury. Respiratory movements were observed fully a minute and a half after the heart had ceased beating. On opening the thorax the heart was found distended with blood and motionless, general contractions were produced on exposing it to the air. The left cavities contained scarlet-coloured blood.

The short time (only five seconds) which occurred between the injection of this substance, and the cessation of the action of the heart, led me to suspect that it might possibly affect that organ, by acting in the nervous system even before it could be circulated over its parietes. I endeavoured to ascertain if this were really the case, by injecting a solution of it directly into the arteries, when the phenomena observed fully proved the unfounded nature of this opinion. In order that the substance might be brought into immediate contact with the brain, and also to avoid the possibility of its finding its way directly to the heart, I inserted a tube into the carotid of a dog, the orifice looking towards the brain. The instrument, as in former experiments, was connected with the femoral artery. Another reason which led me to fix on the carotid for introducing the injection was, that the free anastomoses of this vessel would allow a portion of the injected fluid to pass into the general circulation, if the injection were pushed with some force. An ounce of an infusion of digitalis, of the same strength as that last used, was now quickly injected. The immediate effect of the injection appeared to be the production of great pain to the animal. Ten seconds after the injection had been introduced, the pressure in the arterial system became suddenly increased, the column of mercury rising from five inches, at which it before stood, up to twelve and fourteen inches, considerable oscillations taking place. It was not until forty-five seconds after the introduction of the poison that the heart appeared at all affected. The pulsations were then rendered very slow, ten or twelve seconds elapsing between each pulsation, the pressure in the arteries was soon diminished, the mercury standing at about three inches. The nervous system was evidently much affected, but respiratory movements still occasionally took place. About four minutes after the injection of the poison the heart suddenly stopped beating. The heart retained its irritability after death, contracting readily on being stimulated. I think the increase of pressure in the arterial system, taking place so soon after the injection of the poison, was owing to a portion of it having passed from the carotids into the aorta, and being thus distributed over the body; for supposing it to exert on the capillaries that action, which I think we are fully justified in attributing to it, the circulation of the poison through the brain could not have taken place to any amount.

Remarks on Tobacco, &c.—The only remark I have to offer on this class of substances is the impossibility of accounting for the great increase of pressure in the arteries, caused by their introduction into the circulation, without allowing them to possess that property which has been selected as the ground of classification, viz. that of modifying the capillary circulation.

Remarks on Euphorbium, &c.—This increase of pressure evidently cannot depend on any change effected in the action of the heart, as we find it taking place before that organ is affected. The sudden cessation of the action of the heart, when these substances were applied to it in at all a concentrated state, also admits of an explanation, by supposing the capillary circulation over the parietes of this organ to become so obstructed, that, for a short space of time at least, sufficient blood was not sent over its parietes to keep up its irritability. In endeavouring to localize the action of these substances, two propositions present themselves; viz. is their action on the capillary circulation the result of their direct application to the inner surface of these vessels, or does it follow from some peculiar action they exert on the nervous system? From a review of the above experiments, I am inclined to believe that these substances act directly on the capillaries themselves; and from the great oscillations observed in the pressure in the arteries, it appears that they also exert a power of modifying the elasticity of the larger vessels. It would be useless to attempt to enter into any explanation of the manner in which the two last classes of substances produce their effects. I shall merely remark, that, though differing considerably in their physical and therapeutical properties, yet none of them, as far as I am aware of, exert any marked effect on the composition of the blood. Should they act by producing such changes, these must be widely different from those caused by the first class of substances, and are such as our present knowledge of organic chemistry does not enable us to detect.

4. I have still a few remarks to offer on the action of one or two substances, which I have not been able to arrange in either of the above divisions. These are morphia, cantharides, and nitric acid.

Action of Morphia.—Morphia when introduced into the veins shows its effects on the heart, by rendering its pulsations slower, the pressure in the arterial system becoming diminished. The injection of ten grains of acetate of morphia into the jugular of a dog was followed by a diminished action of the heart.

Action of Cantharides.—Cantharides, although so different from morphia in its therapeutical properties, yet, when introduced into the blood, has the same effect on the heart, in rendering its pulsations slower, and causing a diminution of pressure in the arteries, which is not followed by any sudden increase of pressure, as in the last class of substances.

An infusion from two drachms of the flies was introduced into the veins, without apparently causing much inconvenience to the animal. It produced some dyspnœa when passing through the lungs, and slight irregularity in the action of the heart for a few seconds.

Action of Nitric Acid.—The following experiment with nitric acid affords us an instance of death, produced apparently by obstruction to the passage of the blood through the lungs.

A drachm of nitric acid, mixed with an ounce of water, was injected into the jugular vein of a dog. Twenty seconds after the injection, violent struggles were produced, and all respiratory movements soon ceased. A minute after the introduction of the acid, the column of mercury indicating the pressure in the arterial system had diminished from seven inches to one inch and a-half. The animal was now apparently dead, but partial efforts to vomit were observed two minutes later. The thorax was opened four minutes after the injection of the poison. The left side of the heart was still pulsating. The right side was distended with blood to the greatest extent possible. On opening it, its contents were found to be quite solid, resembling blood which had been boiled. This solid blood would be traced as far as the second divisions of the pulmonary artery in one direction, and extended the whole length of the descending cava, and a short distance into the iliacs. The left side of the heart contained a small clot of scarlet blood, of the natural colour and consistency.

Action of the Nervous System.—I shall now conclude the detail of these experiments by a short notice of one or two which I have performed, in order to ascertain the direct influence of the nervous system on the heart. I am sorry that, from the many sources of fallacy which the severity of the operation introduces in the performance of these experiments, I am unable to bring the subject forward in a more complete state. In those experiments in which I have obtained positive results, I find that the sudden destruction of the lower part of the spinal chord (from the lower part of the dorsal portion to the end) is not followed by any direct action on the heart, or diminution of the pressure in the arteries. I have also destroyed the portion extending from the lower part of the dorsal to the upper part of the cervical region, without producing any direct effect on the heart, that organ gradually stopping from the asphyxia which was produced by the operation.

I have now brought to a conclusion the detail of the experiments performed by me with a view to ascertain the mode in which various agents, chemical and pharmaceutical, act, when introduced into the veins. I shall not presume to occupy your time by attempting to enter into any theoretical deductions from them, which would come with a bad grace from one who is still engaged in studying the elementary branches of his profession. My aim has been to render these notices as strictly experimental as possible, and to bring forward a few facts which might serve to call the attention of more able inquirers than myself to a subject, the investigation of which is, I think, likely to throw much light on many interesting questions in physiology. And it is this which must serve as my apology for bringing the subject forward in so unfinished a form.

ART. III.—*An Appendix to the Fourth Edition of the Anatomy of the Human Body.* Printed separately for the use of those who have the former editions. By W. CHESELDEN, Surgeon to her Majesty, F. R. S., and Surgeon to St. Thomas's Hospital. London. Printed by W. Bowyer: and sold by James and John Knapton, A. Bettesworth, J. Osborn and T. Longman, J. Noon, and J. Clark, at the Royal Exchange. 1730.

WE had occasion, lately, to direct the attention of our readers to the singular misapprehension relative to the particular nature of Cheselden's improved lateral operation of lithotomy, which was pointed out by Dr Yelloly, some years since, in a letter to Sir Astley Cooper, that was published in the fifteenth volume of the Medico-Chirurgical Transactions.

The means of completely clearing up the point in question, after the misconception of a century, arose, in a material degree, from Dr Yelloly being so fortunate as to obtain access to an Appendix to the fourth edition of Cheselden's Anatomy, which, in the course of his inquiries, he found to be referred to. We have therefore thought it might be desirable to give a reprint in our Journal of this interesting tract, which is exceedingly scarce; and are happy in being able to do so, by the good offices of Dr Yelloly, in obtaining for us, through the kindness of Dr Clutterbuck, from the library of the Medical Society of London, the use of the copy which he employed in his letter to Sir Astley Cooper. Though some parts of this Appendix have been introduced into subsequent editions of Cheselden's Anatomy, yet, as the tract is very short, we have thought it would be most acceptable to the public for us to give it entire.

We append some observations for the purpose of making the important bearings of Cheselden's Appendix the more obvious.

CHAP. I.—*A Short Historical Account of Cutting for the Stone.*

The most ancient way of cutting for the stone, is that described by Celsus, now called cutting by the gripe, or the lesser apparatus; and as Celsus relates, was not performed in his time but upon boys between nine and fourteen years of age. But Rosset, in his treatise *de partu Cæsareo* says, he thinks this must be a mistake in the manuscript from which Celsus was printed, seeing younger boys are well known to be much fitter subjects for that operation. But this way of operating is very difficult in men, and perhaps attended with inconveniences if it has ever been practised, that the operators have not thought fit to publish.

In the year 1524 Marianus published the method of cutting by the greater apparatus, (now commonly called the old way) which was invented by his master Johannes de Romanis; this operation has one advantage over the former, that it may be done conveniently upon men full grown.

In the year 1697 Frere Jacques came to Paris (as Mery and others have related) to practise his new invented way of cutting for the stone, which he declared to be directly into the bladder, and which he had performed with so great reputation, that the king ordered him to cut in both the hospitals, where though some of his patients recovered surprisingly, yet the major part miscarrying, his operation fell presently into disgrace. Upon opening these unhappy patients (as Mery and others relate) in some the bladders were found cut through, and wounded in many places; in some the neck of the bladder totally divided; and in others the intestinum rectum miserably cut, which upon the whole sufficiently shewed, that the ill success of this operation was not owing to the nature of it, but the operators' ignorance in anatomy, as they themselves insinuate.

In the year 1717-8 Dr James Douglas, in a paper presented to the Royal Society, demonstrated, from the anatomy of the parts, that the high operation for the stone might be practised; (which had been once performed by Franco injudiciously, and by him disrecommended, though his patient recovered; and afterwards strongly recommended, but not practised by Rosset) yet no one undertook it, till his brother, Mr John Douglas, about three years afterwards performed it, and with great applause, his two first patients recovered. Soon after a surgeon of St Thomas's Hospital cut two, who both recovered; but the same gentleman afterwards cutting two, who miscarried by the cutting or bursting the peritoneum, so that the guts appeared, this way immediately became as much decryed, as it was before commended; and the surgeons of St Bartholomew's Hospital who had resolved to do this operation, altered their resolution, and went on in the old way. The next season it being my turn in St Thomas's, I resumed the high way, and cutting nine with success, it came again in vogue; after that every lithotomist of both hospitals performed it; but the peritoneum being often cut or burst, (twice in my practice,) though some of these recovered, and sometimes the bladder itself was burst from injecting too much water, which generally proved fatal in a day or two. Another inconvenience attended every operation of this kind, which was, that the urine's lying continually in the wound sometimes made sloughs, and always retarded the cure, but then it was never followed with an incontinence of urine.

What the success of the several operators was, I will not take the liberty to publish; but for my own, exclusive of the two before mentioned, I lost no more than one in seven, which is more than any one else that I know of could say; whereas in the old way, even at Paris, from a fair calculation of above 800 patients, it appears that more than two in seven died. And though this operation came into universal discredit, I must declare it is my opinion, that it is much better than the old way, to which they all returned, except myself, who would not have left the high way but for the hopes I had of a better, being well assured that it might hereafter be practised with greater success; these fatal accidents having pretty well shewn how much water might be injected, and how large the wound might safely be made.

But hearing of the great success of Mr Rau, professor of anatomy at Leyden, I determined to try, though not in his manner, to cut directly into the bladder; and as his operation was an improvement of Fryar James, I endeavoured to improve upon him by filling the bladder, as in the high way, with water, leaving the catheter in, and then cutting on the outside of the catheter into the bladder, in the same place as upon the gripe, which I could do very readily, and then I took out a stone of any size with more ease than in any other way. I forbear to give a more particular account of the manner of performing this operation, it having been done already as well as is possible by Dr Douglas. My patients for some days after the operation seemed out of danger, but the urine which came out of the bladder continually lodging upon the cellular membrane on the outside of the rectum, made fœtid ulcers, attended with a vast discharge of stinking matter, and from this cause I lost four patients out of ten. The case of one which escaped was very remarkable; a few days after he was cut, he was seized with a great pain in his back and legs, with very little power to move them; upon which he turned upon his face, and rested almost constantly upon his knees and elbows above a fortnight together, having no ease in any other posture all that while, at length his urine coming all the right way, his wound soon healed, and he recovered the use of his back and limbs. I think all these severe symptoms could proceed from no other cause than the urine and matter somehow offending the great nerves which come out of the os sacrum to go to the lower limbs. I then tried to cut into the bladder, in the same manner that Mr Rau was commonly reported to do, but there had the same inconvenience from the urine's lodging upon the cellular membrane on the outside of the intestinum rectum. Upon these disappointments I contrived the manner of cutting, which is now called the lateral way.

This operation I do in the following manner. I tie the patient, as for the greater apparatus, but lay him upon a blanket several doubles upon an horizontal table three foot high, or a little more, with his head only raised. I first make as long an incision as I well can, beginning near the place where the old operation ends, and cutting down between the musculus accelerator urinæ, and erector penis, and by the side of the intestinum rectum. I then feel for the staff, and cut upon it the length of the prostate gland strait on to the bladder, holding down the gut all the while with one or two fingers of my left hand. The rest of this operation is the same as in the old way. But in this way there being often cut small vessels, I always tie them with a ligature, passed under them by the help of a crooked needle.

The first twenty-seven patients cut this way recovered, and I believe are all living at this time. Indeed I had cut thirty-one who recovered before I had one died, having cut four more who recovered between the time the twenty-eighth was cut, and the time he died; but I scorn to use any fallacious way of representing my success. Some of these being cut in the hospital, and some privately, the truth of this account may be suspected by those who do not know me. I cannot take the liberty to mention the names of private patients, therefore I will give a detail of those only which I cut this way in the hospital, where the first twenty-five recovered, to the truth of every one of which I had above twenty witnesses, and I do believe these patients are all living at this time.

MARCH 27, 1727.

Robert Kason	aged 4
Henry Webb	5
Francis Willsmore	15

APRIL 12, 1727.

Hannibal Basketfield	3
Thomas Hull	4
Alexander Montgomery	8
Henry Cope	44

MAY 15, 1727.

Thomas Nailer	7
John Letheridge	8
Daniel Bezely	9

APRIL 8, 1728.

Walter Bromingham	4

William Jersey	aged 4	
Thomas Kennet	13	
April		
William Davis	4	
Thomas Ellis	5	
William Adams	6	
James Bond	10	
May 9, 1728.		
John Parson	5	
William Chater	11	
Wilfrey Peale	40	
William Haffenden	67	
May 25, 1728.		
Joseph Godwin	3	
Ellis Bakewell	5	
March 21, 172$\frac{8}{9}$.		
William Ward	10	
John Edwards	15	
Thomas Warren	17	died.
Isaac Wood	25	
April 21, 1729.		
John Payne	4	
Thomas March	6	
Robert Caruthus	10	
April 29, 1729.		
Gabriel Forster	21	
Simon Sutcliffe	36	
John Miles	42	

MAY 1729.

Four cut in the presence of Mons. Morand, one of which, named Money, died. The names of the other three I have lost.

July 1, 1730.	
Henry Hall	4
Walter Scott	4
John Tooting	7
John Paxter	11
Edward Eilding	13

Edinr Med. & Surg. Jour. TABLE I. p. 350 & 353, Vol. LI

CHESELDEN'S 21ST CASE.

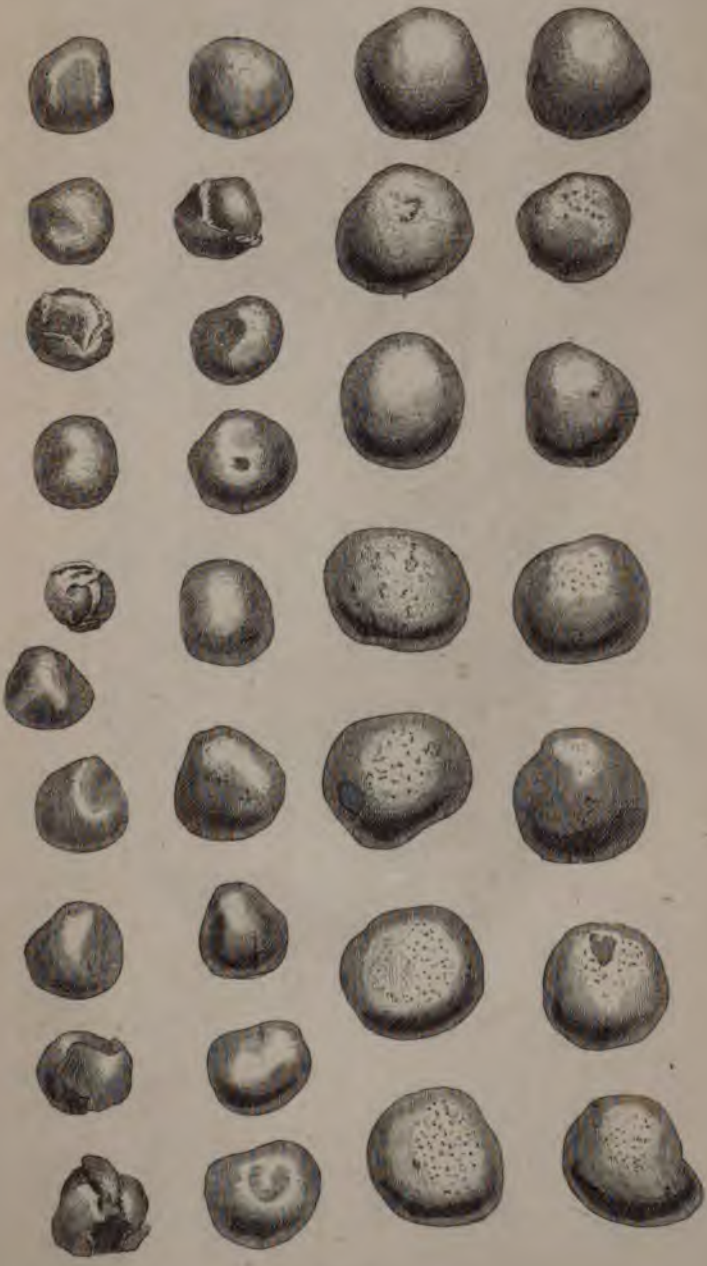
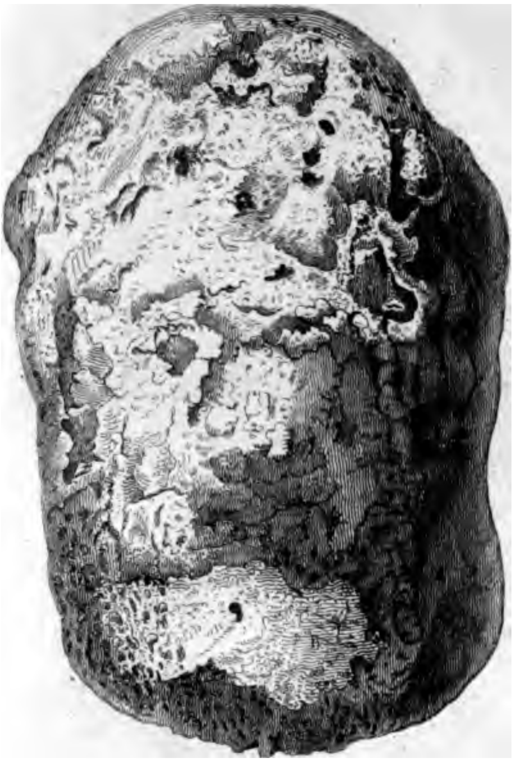
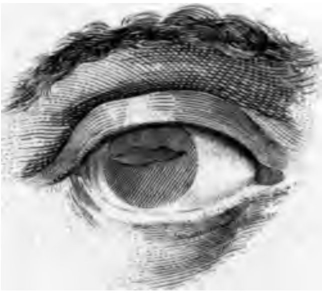


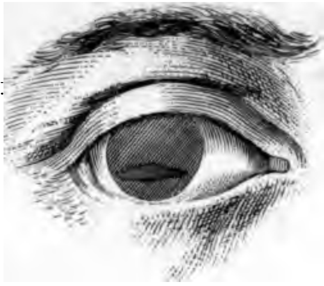
TABLE II.

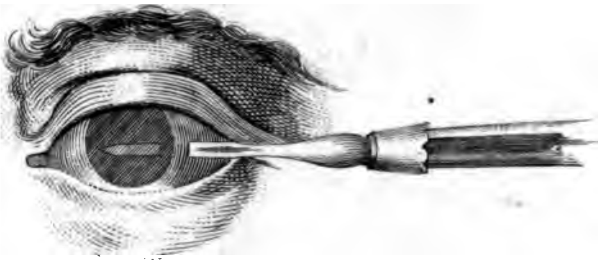
VIEW OF STONE IN CHESELDEN'S 33D CASE.



Med. & Surg. Jour. *TABLE III.* *p. 353*







JULY 31, 1730.

Joseph Wright	aged 6
Joshua Philips	7
Richard Michell	10
Daniel Hall	14

In all 46.

Thomas Warren, the first who died, had been troubled with the stone from his cradle, to the time he was cut, seventeen years old; he was very much wasted with pain, and had bad kidneys; he lived free from pain three weeks after he was cut, but his wound never digested well.

The other who died, named Money, had a violent whooping cough. He was removed from the hospital for the benefit of the air, and died a fortnight after the operation. Many of the children had the small-pox during their cure, and some the measles.

CHAP. II.—*An account of some Observations made by a young gentleman who was born blind, or lost his sight so early, that he had no remembrance of ever having seen, and was couch'd between thirteen and fourteen years of age.*

Though we say of this gentleman that he was blind, as we do of all people who have ripe cataracts, yet they are never so blind from that cause but that they can discern day from night; and for the most part in a strong light, distinguish black, white, and scarlet; but they cannot perceive the shape of any thing; for the light by which these perceptions are made, being let in obliquely through the aqueous humour, or the anterior surface of the chrystaline (by which the rays cannot be brought into a focus upon the retina) they can discern in no other manner, than a sound eye can through a glass of broken jelly, where a great variety of surfaces so differently refract the light, that the several distinct pencils of rays cannot be collected by the eye into their proper foci; wherefore the shape of an object in such a case cannot be at all discerned, though the colour may. And thus it was with this young gentleman, who though he knew these colours asunder in a good light, yet when he saw them after he was couch'd, the faint ideas he had of them before, were not sufficient for him to know them by afterwards, and therefore he did not think them the same which he had before known by those names. Now scarlet he thought the most beautiful of all colours, and of others the most gay were the most pleasing, whereas the first time he saw black it gave him great uneasiness, yet after a little time he was reconciled to it: but some months after, seeing by accident a negro woman, he was struck with great horror at the sight.

When he first saw, he was so far from making any judgement about distances, that he thought all objects whatever touched his eyes, (as he expressed it) as what he felt did his skin, and thought no objects so agreeable as those which were smooth and regular, though he could form no judgement of their shape, or guess what it was in any object that was pleasing to him. He knew not the shape of any thing, nor any one thing from another, however different in shape or magnitude; but upon being told what things were, whose form he before knew from feeling, he would carefully observe, that he might know them again; but having too many objects to learn at once, he forgot many of them; and (as he) said at first he learned to know, and again forgot a thousand things in a day. One particular only (though it may appear trifling) I will relate. Having often forgot which was the cat, and which the dog, he was ashamed to ask; but catching the cat (which he knew by feeling) he was observed to look at her steadfastly, and then setting her down, said, so puss! I shall know you another time. He was very much surprized, that those things which he had liked best, did not appear most agreeable to his eyes, expecting those persons would appear most beautiful that he loved most, and such things to be most agreeable to his sight, that were so to his taste. We thought he soon knew what pictures represented, which were shewed to him, but we found afterwards we were mistaken; for about two months after he was couched he discovered at once they represented solid bodies, when to that time he considered them only as party-coloured planes, or surfaces diversified with variety of paint; but even then he was no less surprized, expecting the pictures would feel like the things they represented, and was amazed when he found those parts, which by their light and shadow appeared now round and uneven, felt only flat like the rest, and asked which was the lying sense, feeling, or seeing?

Being shewn his father's picture in a locket at his mother's watch, and told what it was, he acknowledged a likeness, but was vastly surprized; asking, how it could be, that a large face could be expressed in so little room, saying, it should have seemed as impossible to him, as to put a bushel of any thing into a pint.

At first, he could bear but very little sight, and the things he saw, he thought extremely large; but upon seeing things larger, those first seen he conceived less, never being able to imagine any lines beyond the bounds he saw; the room he was in he said, he knew to be but part of the house, yet he could not conceive that the whole house could look bigger. Before he was couched, he expected little advantage from seeing, worth undergoing an operation for, except reading and writing; for he said, he thought he could have no more pleasure in walking abroad than he had in the garden, which he could do safely and readily. And even blindness he observed, had this advantage, that he could go any where in the dark much better than those who can see; and after he had seen, he did not soon loss this quality, nor desire a light to go about the house in the night. He said, every new object was a new delight; and the pleasure was so great, that he wanted ways to express it; but his gratitude to his operator he could not conceal, never seeing him for some time without tears of joy in his eyes, and other marks of affection: And if he did not happen to come at any time when he was expected, he would be so grieved, that he could not forbear crying at his disappointment. A year after first seeing, being carried upon Epsom Downs, and observing a large prospect, he was exceedingly delighted with it, and called it a new kind of seeing. And now being lately couched of his other eye, he says, that objects at first appeared large to this eye, but not so large as they did at first to the other; and looking upon the same object with both eyes, he thought it looked about twice as large as with the first couched eye only, but not double, that we can any ways discover.

I have couched several others who were born blind, whose observations were of the same kind; but they being younger, none of them gave so full an account as this gentleman.

TABLE I.—Thirty three stones taken from William Haffenden, in the sixty-eight year of his age. The next day after he was cut, the measles appeared, which he had in a very severe manner. He is now living at Gravesend. (Vid. page 350, line 12th.)

TABLE II.—A stone which weighed eleven ounces, and measured ten inches round, taken from John Miles, who is now living at Reading. (Vid. p. 350, l. 27.)

TABLE III.—Three figures of eyes to explain an operation, which I invented some years ago, and printed a short account of in the Philos. Trans. and have often practised with success. The distemper for which this operation is performed, is either a total closure of the pupil, which is sometimes natural, and sometimes happens from inflammation; or else when the pupil is extremely contracted, and the inner edges of the iris growing to a cataract, or part of a cataract after couching. The manner of doing this operation is thus: the eyelid being firmly held open by an instrument, a small knife or needle, edged on one side, is thrust through the tunica sclerotis, as in the lower figure; and then forwards through the iris, the edge being turned

to the iris; in drawing of it out, a slit is cut as in the two upper figures. When this distemper is without a cataract, it is best to make the operation in the middle, as in the upper one; but if there is a cataract, or part of a cataract, then to make it higher, that the cataract may not obstruct the light. These cataracts are generally very small, and sometimes by reason of their adhesion not to be removed. The aperture in the middle eye, was made lower than the centre of the cornea, there being an albugo on the upper part of it, which made it unfit to perform the operation in that part.

From the information communicated in Cheselden's preceding account of the operation of lithotomy, which was introduced into the subsequent editions of his Anatomy, it appears, that after the high operation for the stone was discontinued, Cheselden made several attempts to improve the lateral operation, of which an account is given in a work published by Dr James Douglass, a distinguished anatomist of that period, in the year 1726. *

The statements contained in this work appear to have been correct; for in relation to one of the attempts at improvement, Cheselden observes, that an account of it has been given, "as well as is possible, by Dr Douglass."

Thus far, therefore, Dr Douglass may be regarded as a competent authority on the subject; but in the year 1731, he published an Appendix to his History of the Lateral Operation, which is totally irreconcilable with the accounts published relative to it by Cheselden himself. †

We shall take from Dr Yelloly's letter to Sir Astley Cooper, some of the principal particulars of evidence connected with his correction of Dr Douglass's singular misapprehensions relative to the nature of Cheselden's improved lateral operation, which led to a very long period of misapprehension on the subject.

The account given by Dr Douglass of Cheselden's operation is as follows. After making the external incision, Cheselden introduces "the fore-finger of the left hand into the wound, and keeps it there till the internal incision is quite finished, first to direct the point of the knife into the groove of the staff, which

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* History of the lateral operation, or an account of the method of extracting a stone, by making a wound near the great protuberance of the *os ischium*, through the common integuments and *levator ani* into the side of the bladder, without touching the urethra, prostate gland, *vesiculæ seminales*, or any other of the urinary or seminal vessels; first attempted by Frere Jacques in France, and afterwards successfully performed by Professor Rau in Holland; with a postscript concerning the introduction and improvement of this method tried in London. 4to. London, 1726.

† Appendix to the History of the Lateral Operation for the stone, containing Mr Cheselden's present method of performing it. 4to. London, 1731.

he now feels with the end of his finger, and likewise to hold down the *intestinum rectum*, by the side of which his knife is to pass, and to prevent its being wounded. This inward incision is made with more caution and more leisure than the former.

"His knife first enters the groove of the rostrated or strait part of his catheter, *through the sides of the bladder*, *immediately above the prostata*, and afterwards the point of it continuing to run in the same groove in a direction downwards and forwards, or towards himself, he divides that part of the sphincter of the bladder that lies upon that gland, and then he cuts the outside of one-half of it obliquely, according to the direction and whole length of the urethra that runs within it, and finishes his internal incision by dividing the muscular portion of the urethra on the convex part of his staff." *

On comparing this account with Cheselden's own words, as given in the preceding Appendix, an entire discrepancy between the modes of operation described by them is perfectly obvious. But the misapprehension into which Dr Douglass appears to have fallen in respect to Cheselden's operation, is the more extraordinary, because not only was Cheselden's Appendix published the year before Dr Douglass wrote his own Appendix, but it was quoted by the latter for the purpose of showing that the mode of operating described by Cheselden had been discontinued. This is apparent from Dr Douglass's further observations; for he goes on to say, "When he first began to practise this method, Cheselden cut the very same parts *the contrary way;* that is, his knife entered first the muscular part of the urethra, which he divided laterally from the pendulous part of its bulb to the apex, or first point of the prostate gland, and from thence directed his knife upward and backward all the way into the bladder; as we may read in the Appendix he lately published to the fourth edition of his book of anatomy. But some time after he observed, that in that manner of cutting, the bulb of the urethra lay too much in the way; the groove of the staff was not so easily found, and the *intestinum rectum* was in more danger of being wounded."

Here, therefore, it appears, that Dr Douglass was actually cognizant of the performance of an operation by Cheselden, similar to that described by the latter, which he states having been given up for that of which he himself gives an account. This, however, could not have been the case; for M. Morand, the distinguished Paris surgeon, who was sent over by the

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* Appendix to the History of the lateral operation for the Stone, containing Mr Cheselden's present method of performing it, p. 22. 4to. London, 1731.

French government to be instructed in Cheselden's operation describes, in a communication which was made to the Royal Academy of Sciences in 1731, but not published till two years afterwards, Cheselden's operation, by translating, as nearly as possible, that account of it which was published by Cheselden the year before, without giving the least intimation of any change having been adopted in the mode of its performance.

But the most conclusive evidence against such an idea is derived from Cheselden himself, who introduced the account of his improved operation, in nearly the words of his Appendix, into the fifth edition of his Anatomy, published in the year 1740; and into the edition of 1750, (the sixth,) which is the last that appeared during his lifetime. There is great laxity, it may likewise be observed, as to the mode in which Dr Douglass mentions Cheselden's number of operations, and success. He states, that at the time when he (Dr Douglass) wrote, (namely, in July 1731), Cheselden had "saved 50 patients out of 52, whom he cut successively in St Thomas's Hospital."[*] But the fact is, that he lost *three* of the first 50, which is one-half more;[†] and in the 52 mentioned by Dr Douglass, no fewer than 46 came under the category of Cheselden's very operation, which Dr Douglass so singularly persuaded himself had been relinquished.

But the evidence even goes further; for though Douglass states the number operated upon up to the time that he wrote (July 1831) to be 52, it was, in truth, 66, up to the previous April. This is learned by a communication from M. Morand, of April 3d, in which he states that he had just received from Mr Cheselden a list of 20 patients, on whom he had operated since the publication of the 46 mentioned in the Appendix, and that upon the original plan which Dr Douglass assures us had been long previously repudiated.[‡]

Heister seems to have been one of the first authors who took Dr Douglass's representations as demonstrative of what Cheselden's improved operation of lithotomy was; and he has been followed by most of the first surgical writers, both of this and other countries. It is certainly a circumstance not a little remarkable in the history of our profession, that the knowledge of Cheselden's improved operation should have been sought for rather in Dr Douglass's account of it, than his own; and we think, with Dr Yelloly, that this is very much to be attributed to the "reliance which has been placed on Dr Douglass's judgment and opportunities of observation; to the ready credence usually given to confident assertions of historical facts; and to the disinclination which there is either to ascertain, or to investigate, the evidence on which such facts may depend."

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[*] Appendix to the History of the lateral operation for the Stone, containing Mr Cheselden's present method of performing it, p. 2. 4to. London, 1731.

[†] "Of the first 50, only 3 died." Anatomy, p. 332.

[‡] 'Je viens de recevoir la liste des malades de M. Cheselden taillés depuis celle qui est imprimée dans son Appendix, et j'apprends qu'il, en a taillés vingt, dont il en est mort deux. Morand's Recherches sur l'Operation de la Taille; Memoires de l'Académie Royale des Sciences pour l'année 1731, p. 144.

ART. IV.—*Case of Extensive Aneurism by Anastomosis in a child ten months old, involving the branches of the Temporal and posterior Auricular Arteries, treated successfully by the Twisted Suture.* By J. MACLACHLAN, M. D. Assistant-Surgeon 79th Regiment.

PROFESSOR LALLEMAND of Montpellier, in a paper on erectile tumours, published in the Archives Générales for May 1835, gives the history of three cases which he treated with perfect success by incision and the twisted suture. Finding that the irritation occasioned by the needles, contributed materially to the obliteration of the tumours, he abandoned the preliminary incision in a fourth case. The tumour was traversed with 120 needles. In rather less than three months, the adhesive inflammation had converted it like a fibrous tissue, and the cure was complete. This was in December 1834. Since then the practice has been resorted to by a few individuals in this country, but the details of the cases do not appear to have been given to the public.* In France it is more generally employed, and in a paper entitled *Recherches et Observations Cliniques sur les Tumeurs Erectiles*, by A. E. Bouchacourt, in the Revue Medicale for August 1838, a case is related in which an erectile tumour of the size of two fists (" de la grosseur de deux poings,") occupying the left cheek of a young countryman, 18 years of age, surrounded and traversed by large veins, was treated in this manner. On the 12th April 1836, 40 needles were introduced, and on the 9th May following 14 more. The cure was successful. In the following case the practice succeeded beyond my most sanguine expectation, and when the age of the subject, and the nature and extent of the tumour, are taken into consideration, it is not only interesting but highly important and truly satisfactory.

CASE.—A. M., a remarkably fine, stout, healthy, female twin child, belonging to a sergeant of the regiment, aged on the 2d April 1838, ten months, was observed, a fortnight after birth, to have on the right temple a small reddish flat mark, not much larger than a pin-head. The discoloration slowly spread, and gradually rose above the surface. The child was brought to me in the fifth month. At that period there was in front of the right ear, immediately above the zygoma, a tumour possessing the character of aneurism by anastomosis, extending upwards for about an inch, and being half that in breadth. There were several small flat livid spots behind the ear and about the cartilages. In two months the tumour had rapidly increased in every direction. During that time the child had been seen occasionally by my friend Staff-surgeon Mahony. It had ulcerated superficially behind the ear, and there was a copious discharge of purulent matter from the external meatus. Having attained certain dimensions, it appeared to remain stationary. For several months prior to the 2d April, there had been no perceptible increase. The parts behind the ear were, however, constantly breaking out into superficial ulcerated points, from which occasionally there was an oozing of arterial blood.

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* Of this mode of curing erectile tumours by M. Lallemand, a case is recorded in the 45th volume of this Journal, at page 556.

On the 2d of April 1838, the tumour presented the following characters. The tumour appeared chiefly to be arterial. A few enlarged veins encircled and ran through it; but it seemed essentially to consist in an enlargement in size, if not in number, of the branches of the superficial temporal and posterior auricular arteries. Commencing at the root of the zygomatic process, it proceeded directly upwards, reaching nearly the anterior fontanelle, and terminating in a nuckle-like eminence. This, the temporal portion, measured three inches and a-half in length; at its origin it was rather more than an inch in breadth, and, when distended, it rose throughout fully three-fourths of an inch above the surface. The whole of the figured parts of the ear were either of a livid or bright strawberry colour, little if at all elevated in front, but in the angle behind forming a livid flaccid swelling. Immediately in front of the mastoid process, this was of the size of a small filbert. From this point it took a semicircular course backwards, following the direction of a superficial branch of the posterior auricular artery, and extending upwards of two inches. Towards the angle of the lower jaw there was a diffused subcutaneous tumefaction of a bluish cast. The whole of the tumour, with this exception, was of a deep strawberry colour, mammillated, brightening as it distended, and with a bluish tinge here and there. The least exertion in crying produced a remarkable increase in the temporal and occipital portions, and there was a constant thrilling felt on applying the fingers to the temple. There were also two detached, small, circular, flat nævi close upon the orbit, and two or three others of the same nature above the ear on this side, figured in the plate. (Pl. IV.)

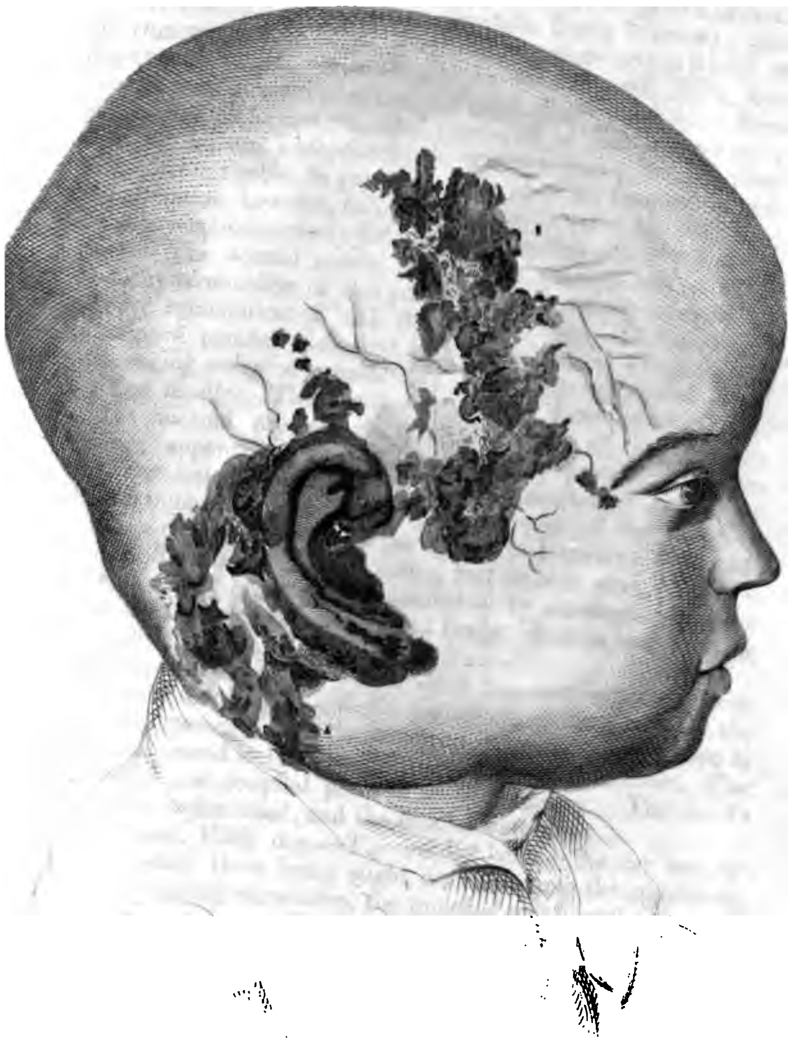
The mother was extremely averse to any operation. I had tried compression upon the temporal portion, though, for want of a proper apparatus, very ineffectually, and the application of ice and astrin-

Edinr Med. & Surg. Jour.

PLATE IV.

p. 358 & 362, Vol. L

DR. MACLACHLAN'S CASE.



G. Aikman, Sc.

gents, as was to have been expected, without the least benefit. On strongly representing to her the great danger of farther delay, and finding my opinion corroborated by others, she at last reluctantly consented to an operation. A consultation, consisting of Sir George Ballingall; Staff-surgeon Mahony; Surgeon Lorimer, 79th Regiment; Mr Fergusson, Surgeon, Royal Infirmary; and Dr Duncan of Edinburgh, unanimously recommended a trial of the twisted suture. Accordingly, on the 2d April 1838, four needles were introduced at twelve o'clock, in the presence of these gentlemen. The first traversed a space of an inch and-a-half in front of the ear, immediately above the zygoma, the intention being, if practicable, to get under the root of the temporal artery. No alteration, however, followed; on the contrary, the whole tumour became enormously distended by the cries and efforts of the child. The second needle was pushed through half an inch from the termination of this portion of the tumour, so as to cut off any communication with the opposite side. The third was introduced parallel with, and half an inch from, the first, this part having enlarged considerably on twisting the thread round the first needle. The fourth was inserted immediately in front of the mastoid process, close to the lobule of the ear, embracing, apparently, the posterior auricular artery. This suture was instantly followed by a most satisfactory diminution of the tumour occupying the posterior part of the helix, as well as a change of colour in the concha and *meatus externus.* The child was soon quieted; and in half an hour after it fell into a sound sleep, interrupted momentarily by startings. Between three and four o'clock it was up and lively. It slept sound during the night.

4th April, Third day.—The child was seen regularly twice a-day since the second, and did not appear to have suffered the least inconvenience from the needles. The temporal portion of tumour appeared to be more flaccid, and of a deeper blue. The mastoideal and occipital portions were stationary. The child's health was undisturbed, and the needles were firm.

6th April, Fifth day.—The needle behind the ear was removed to-day, there being slight discharge from the punctures, with surrounding excoriation, the tumefied parts overlapping and being irritated by it. The remainder were firm. In the neighbourhood of excoriation were several hard nodules, occasioned by effused fibrin, and resembling an external hæmorrhoid. The posterior part of the ear, as also the meatus, were already much reduced. Four more needles were introduced, the situation of which are pointed out in the accompanying plate. On this occasion, waxed thick purse-silk was employed, as being less likely to cut the skin. Several turns of the silk were made over the parts isolated by the needles, by crossing it from one needle to the other. The child cried less, and fell into a sound sleep soon after the operation.

7th April, Sixth day.—Health continued undisturbed. The first and third needles introduced on the second were withdrawn to-day, though firmly fixed. A drop of blood followed the removal of the first. The silk has been allowed to remain. Excoriation behind much deeper.—A weak solution of the sulphate of zinc to be applied.

8th April, Seventh day.—The temporal portion of tumour was less red. The part anterior to the ear was firm, semitransparent, and of a very pale-blue colour, evidently resulting from effused fibrin. The remaining needle was introduced on the 2d. The one near the termination of the temporal portion, the second inserted, was removed to-day. Neither discharge nor ulceration had taken place at either of the points. The child has been lively and in high spirits. Excoriation discharging copiously, but not extending.

11th, The remaining needles were removed, and found slightly oxidized. Behind the ear ulceration is rather deeper, and yesterday the discharge was discoloured with blood. From the surface of the tumour, immediately above the zygoma, issued a slight sanious and purulent discharge, but the parts underneath felt firm. On withdrawing the seventh needle, a drop of scarlet blood followed. The parts embraced by the silk are white, but not ulcerated.

The child was daily visited since last report. Its health continued undisturbed, and there appeared to be a decrease in the whole tumour operated upon; at all events, it was not nearly so much elevated when the child cries, nor is it so bright.

13th, Discharge from ulcerated parts was much increased, and the tract of the first needle in front of the ear was deeper. The whole tumour above this was also superficially ulcerated and discharging thin ichor. The subjacent parts continued firm, and, excepting when the tumour is touched, the child appeared to be free from pain.

In rather less than three weeks from this date the ulcerations had healed under common applications, the solution of the sulphate of zinc, or the white oxyd of zinc ointment. The tumour gradually disappeared, leaving a firm fibrous texture of a whitish colour indented with the tract of the needles, and very closely resembling a superficial burn. That portion behind the ear of a crescentic form, retained for some time its brightness. I was desirous of passing under and through it two or three needles; but the mother, who all along appeared to view the measure adopted as an experiment, offered so many objections that my intention was not carried into effect. This I scarcely regret. At the period I am writing (30th November 1838,) the skin had nearly acquired its natural colour, interspersed only in front and behind the ear with some little red points. The previous extent of the tumour could now hardly be traced, the hair is growing over it, and, as the child advances in years, that portion only in front of the ear will be exposed. There is still a small flaccid livid swelling, rather larger than a pea, in the loose integuments in front of the mastoid process, not, however, under the influence of the circulation; and the mother says it is decreasing.

It would be difficult to find a more satisfactory or successful case than the above. In this instance, at least, Lallemand's method effected all that could be desired. It is obvious that no other could have been attended with so little risk. Excision was out of the question; the boldest operator, and the most dexterous dissector, would scarcely have attempted the knife. Ligature of the carotid artery was a measure uncertain in result. Although Mr Travers succeeded by tying the carotid in a case of aneurism by anastomosis in the orbit, the very free inosculation in the scalp appears to be unfavourable when the tumour is situated there; and it has been remarked by Mr Phillips,* "that ligature of the carotid has never succeeded in one of the cases in which it has been employed by Pellétan, Mussey, and Wilhaume, for the cure of erectile tumours affecting the temporal fossa. The case related by Dr Mussey, in the American Journal of Medical Sciences, February 1830, and transcribed in the London Medical Gazette, Vol. vi. p. 77, in which he tied both primitive carotids for a large aneurism by anastomosis, on the vertex, and had in six weeks after to extirpate the tumour, is not in favour of the ligature. In a case operated upon by Dupuytren also, in which he tied the carotid for a large nævus situated about the ear, and including the whole texture of the surrounding parts, the operation was equally unsuccessful. It appears to be more certain in cylindroid dilation of the arteries. Mr Syme and others have recorded cases in which they succeeded; but, as is observed by Lawrence, "tying the arteries, or the main trunks of the vessels which supply the part of the body in which nævi are situated, cannot, according to our present experience, be much relied on." Extirpation and ligature of the carotid being rejected, the only remaining method that appeared at all applicable was that proposed and executed by Mr Fawdington of Manchester, the seton; but the purely arterial nature of the tumour seemed to render it hazardous, and the needles were employed, as being less likely to be followed by hæmorrhage.

A remarkable circumstance in the history of this case is, that very few needles, eight only, were sufficient to obliterate the tumour. The repeated ulcerations behind the ear seemed to be an effort of nature to destroy the disease, requiring only a little assistance from art to perfect the cure. Another point worthy of remark, was the total absence of constitutional irritation from the needles. Aware of the danger of punctured wounds in the scalp, I was rather apprehensive of an attack of erysipelas; and, as the needle behind the ear must have embraced some branches of the *portio dura*, there were grounds for anticipating other disagreeable consequences. However, no constitutional effect whatever followed the insertion of the needles on either days. The operation was completed without the loss of a single drop of blood; and the tumour was removed with little or no deformity. These, then, appear to be the principal advantages of the treatment of aneurism by anastomosis, or nævi, by the twisted suture; viz. simplicity; little or no pain in its performance; little or no constitutional disturbance; little deformity, perhaps less, if necessary, except the seton, than by any other method; and little or no risk of hæmorrhage. On this last point Lallemand remarks, "the fear of hæmorrhage is groundless. Immediately after the needle is introduced it fills the wound it has made, and in a very short time inflammation changes the nature of the surfaces divided, and hæmorrhage becomes impossible."

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* London Med. Gaz. Vol. xii.

I employed fine darning-needles, previously prepared with wax-heads, to handle them by. Although Lallemand appears to have used needles, he recommends pins, such as are used for transfixing insects, as being more easily cut or broken off than sewing-needles. He considers thread unnecessary;—" it prolongs the operation, does not increase the inflammation, nor is it necessary to prevent hæmorrhage." It appears to me, however, to assist the progress of the cure; by compressing the vessels, it induces adhesive inflammation in their sides; they are more readily obliterated, and the nature of the tumour is essentially changed, though the irritation occasioned by the presence of the needles seems to be the chief source of benefit.

Successful, however, as the twisted suture has been in the case of nævi and aneurism by anastomosis, like every other method employed, it has occasionally failed. In two cases seen by M. Bouchacourt, the twisted suture practised twice was without benefit; yet it is well deserving a trial. In a considerable majority it will be found to answer; and it is less objectionable than most plans.

Explanation of the Plate (IV.)

The Plate exhibits the extent and form of the tumour, and the situation and relative positions of the needle.

Richmond Barracks,
Dublin, 30*th November* 1838.

ART. V.—*On Division of Stricture of the Rectum high up in the Gut.* By R. A. STAFFORD, Surgeon to the St Marylebone Infirmary.

IN the year 1831, I divided a stricture of the rectum high up in the gut, which came under my care at the St Marylebone Infirmary, and the success of the operation was so complete that I determined, should another case of the same description occur in my practice, to pursue the same treatment. The opportunity of doing so, however, has not been afforded me until lately; and so satisfactory has been the result of the second operation, that I am induced to relate this, as well as the former case, with the view, if not of introducing a new method of treatment, at least of strengthening the utility and importance of a practice not commonly followed when the contraction takes place above the sphincter muscle of the rectum. In the performance of the operation for stricture of the rectum high up in the gut, the posterior part of the contraction should be divided, for the following reasons,—1. There is less danger of wounding the hemorrhoidal, and other important blood-vessels, whereby hemorrhage might result. 2. The peritoneum, which only covers the bowel anteriorly on its upper two-thirds, is not so likely to be injured, from which peritonitis might ensue; and 3. There is not the same risk of cutting through the coats of the intestine, and thus letting out fæcal matter, the consequence of which would be attended by extreme danger.

In addition to these reasons, the sacrum is a support to the posterior part of the bowel, and the rectum being connected to it by the meso-rectum, and loose cellular tissue, is, as it were, fixed, and is not so liable to be affected by the fæces as they pass through it, either by the stretching that might arise by their accumulation, or the action of the muscles by their expulsion.

Under the most favourable circumstances of stricture of the rectum high up in the gut, and even when the part is divided posteriorly, the greatest care and caution is required,—indeed so much is necessary that the contracted part ought to be incised with the the same care as the inguinal and femoral ring in strangulated hernia. The operator, therefore, before he makes the incision, should first ascertain, by passing the finger up the rectum, whether he can discover any artery beating under it, so that he may avoid it. Having done so, the point of the finger should be allowed to remain at the stricture, and a probe-pointed bistoury, or Sir Astley Cooper's knife, should be passed carefully along it up the part. When the cutting edge has passed through the contraction, an incision should be carefully and gently made posteriorly; and the knife withdrawn.

The operator should again pass his finger through the part, and feel if he can discover any artery pulsating beneath it, and, if the stricture is not sufficiently divided, another incision should be made cautiously, until it is perfectly freed; being careful at the same time not to cut through the coats of the intestine.

There may be some cases of stricture of the rectum so high up in the gut that the finger cannot reach the contracted part. In these the operation I have just described cannot be performed. An instrument, therefore, whose semilunar blade can be thrown out laterally at its point, may be employed. The point of this instrument is first passed through the contraction; the blade is then thrown out, pressing on the stilette at its handle, and then it is drawn back through the stricture, whereby an incision is effected.

The cases of stricture high up in the gut, which appear to me to demand the operation, are the following: 1*st*, When the contraction has become so small, (as in the cases which will presently be related,) that the fæces cannot pass, and there is an accumulation in the intestines threatening inflammation of their coats, or of the peritoneum covering them. 2*d*, When the stricture will not yield to dilatation; and 3*dly*, When, from the unyielding and hardened state of the stricture, a carcinomatous ulceration of the part is threatened. In such cases, I am disposed to think no other means can be employed for the relief of the patient. In the first of them, when the patient has, either from neglect, or not being aware of the existence of the disease, allowed the part to contract, so that the expulsion of the fæces is impossible; there is no time for the common means of treatment; the relief must be immediate, or death will take place. In the second, where the stricture will not yield to dilatation, so much mischief takes place along the course of the intestines, causing symptoms of so distressing a nature, diarrhœa, inflammation, and ulceration of the mucous membrane, &c. that to check them is of the utmost consequence. Under such circumstances, we have no other means but division of the part; and in the third, where carcinomatous ulceration is likely to occur, the freeing the part, and thus relieving the irritation kept up in the most likely way; although it cannot be depended upon with certainty, of preventing the mischief.

In support of the opinion I have now advanced, I find that Wiseman recommended this plan of treatment; but I cannot find any cases recorded, excepting one by Mr Mayo, and although he was successful in the case, he considers the operation dangerous. As far as my own experience goes, I am led to think that, with proper care and caution, and by following the directions laid down in the former part of this paper, little mischief is likely to result from it; on the contrary, the greatest possible benefit. I shall now relate the cases which have occurred under my own observation.

Case I.—Susan Webb, a delicate slightly made woman, aged 31, was admitted into the St Marylebone Infirmary, on the 14th of December 1831. She stated, that, about ten years ago, she had *fistula in ano*, which was neglected. A year or two having elapsed, she found she was unable to void the fæces as well as usual, and this difficulty increased until the circumference of the excrement was no larger than the little finger. In the year 1827, she was admitted into St Bartholomew's Hospital, where bougies and other remedies were employed for three months, when she was discharged, having experienced some relief. From that time, however, her complaint gradually returned; and, as she took no means to prevent it, the stricture became nearly closed up again, and she suffered from it the most distressing symptoms. She was always in pain in the rectum, having a constant inclination to go to stool, without being able to evacuate the contents of the bowel. She had frequent tenesmus, accompanied with diarrhœa. There was tenderness in the abdomen on pressure; pain in the back about the region of sacrum; constant flatulency, and sense of fulness; and a continual inclination to vomit. Her pulse was quick, (from 120 to 130 in the minute,) irritable, and feeble. Her tongue was coated by a brown fur in the centre, and the edges and tip (denoting mucous irritation of the alimentary canal,) were of a brilliant red colour. She was greatly emaciated, and so weak that she was obliged to remain in bed. On examining her, a stricture of the rectum was discovered about two inches and a half from the termination of the bowel. It could be distinctly felt, and it gave the sensation to the finger of its being of an indurated stricture. It could only admit through it a No. 12 urethral bougie. Bougies having been passed for more than three months without the least dilatation of the stricture, and other remedies, such as repeated injections, hyoscyamus and opium suppositories, &c. having been employed with no benefit, I proposed, as the symptoms were increasing, and evidently, from the pain and tenderness of the abdomen, chronic peritoneal inflammation was present,—that the stricture should be divided. At this time the sufferings of the patient were so great, and the pain and irritation occasioned by the obstruction of the fæces so distressing, that she was fast sinking. She was constantly on the night-table without accomplishing the desired object; she never slept; and was always in the apprehension of immediate death.

February 25, 1832.—An instrument about eight inches in length, of a slightly curved form, and, at the side of whose point on its convex surface, a semilunar blade could be thrust out, was introduced up the rectum through the stricture. Immediately on having passed the contracted part, the blade was protruded and drawn back through the stricture, by which it was divided at that part of the bowel immediately in contact with the sacrum. T middle finger could now be easily passed through the contrac part, and a bougie of the same size was introduced, and left th for a quarter of an hour. She could not, however, bear it t remain longer, in consequence of the irritation it produced. I was therefore withdrawn. The operation caused but little pai and only a very few drops of blood followed the incision. In th evening she had a motion which passed with more freedom th for years.

26th, She spent a tolerable night. The fæces had passed freely. She still feels pain in the rectum, as high as where the gut pass over the edge of the sacrum. She has slight tenderness on pressure of the abdomen; pulse 90; tongue red and aphthous in som parts; discharge of pus with the fæces. An opiate and fomentation were ordered.

27th, Not worse; if anything rather easier.—The opiate was repeated.

28th, Much the same as yesterday. The bowels acted freely.

March 1st, She still feels pains in the rectum opposite the upper part of the sacrum, and has tenderness on pressure of the abdomen.—Leeches to the epigastric region, and an opiate every four hours were ordered.

2d, The pain is nearly gone; tenderness very slight; a No. 6 rectum bougie was passed with great ease; pulse 90; tongue still red; the aphtha diminished.—The powders were continued.

3d and 4th, The pain is quite gone; and she feels stronger.

5th, Until to-day doing well. The bowels are confined, not having had a motion for twenty-four hours; pain returned; abdomen tender on pressure.—Leeches, castor oil, and an opiate at night were ordered.

6th, Not quite so well; pain increased; bowels freely open.—Leeches to the epigastric region.

7th, Pain in the abdomen lessened; bowels open without difficulty; stools watery, yellow, with some purulent matter. She complains of acidity in the stomach. An alkaline draught to be taken.

8th, The pain is gone; pulse 90; tongue moist, and slightly furred; aphtha gone.

From this time until the 20th she went on well. Her fæces were discharged without difficulty and copiously; the pain of the abdomen was gone; a large bougie was passed with ease, and with but little pain; and the patient felt in all respects better. On the 20th, however, she was seized with erysipelas of the face, which began on the nose, and spread all over the head. In spite of every remedy that could be employed, she went on from bad to worse; and died on the 25th of March at 1 o'clock in the morning from the erysipelas.

Examination of the body after death.—The head and face were considerably swollen from the erysipelas. The arachnoid membrane of the brain was inflamed, and under various parts was effused serous lymph. The substance of the brain was reddened, with bloody points throughout its whole structure. About one ounce of serous fluid was found in each of the lateral ventricles.

The viscera of the thorax were healthy. The liver was tuberculated; the stomach healthy; adhesions and a slight blush of red were observed in parts of the peritoneum.

The mucous membrane of the small intestines was tolerably healthy; but evident marks of former inflammation, and here and there the cicatrix of an ulcer which had healed, were recognized.

Throughout the whole course of the large intestines there were the cicatrices of ulcerations which had healed. In the sigmoid flexure there still existed inflammation of the mucous membrane, with some ulcers which had healed, and others which had not. The gut at the strictured part would admit through it a bougie as large as the thumb. The cicatrix of the wound made by the incision was very apparent, and it was covered by a membrane analogous to that lining the intestine.

The dissection in this case proves, that, had not the patient been attacked by erysipelas of the head and face, she would most probably have recovered. It may be observed, that there had been disease throughout the whole course of the mucous membrane, both of the small and large intestines. In those of the small, along their whole extent, the ulcers had healed, and the inflammation had subsided. In the large intestine, also, the ulcers had healed, excepting some in the sigmoid flexure. It is more than probable, therefore, the strictured part being so much opened, and the exciting cause removed, that, in progress of time, those remaining would have healed also.

CASE II., August 18, 1837.—Henrietta Wilmot, a spare, thin, haggard-looking woman, aged 35, a patient in the St Marylebone Infirmary, laboured under the following symptoms;—Extreme pain and tenderness of the epigastric region, accompanied with very great distension of the whole abdomen, which felt hard and tumid. She suffered from violent and continued vomiting, and had frequent tenesmus without voiding fæcal matter. Her pulse was rapid, hard, and wiry. Her tongue was dry and black, and her countenance expressive of extreme distress. No food or medicine remained on the stomach; and as there was no external cause, such as hernia, &c. to account for the symptoms, she was questioned as to the state of the rectum. She acknowledged that, for some days past, no stool had taken place, and that, for more than two or three years, she had felt difficulty in voiding her motions. On examining the rectum, I found a stricture nearly three inches from the anus. It was annular, and felt extremely indurated; and it would only admit through it, upon trial, a No. 10 urethra bougie. I immediately proposed division, feeling it to be the only chance of relief; and in the presence of my friends, Mr Beaumont and Mr Clay, I operated. Having passed the point of my finger up to the stricture, I introduced along it Sir Astley Cooper's knife, and having passed the cutting-edge through the contraction, I divided it posteriorly towards the sacrum. This was accomplished without difficulty; but, on passing the finger through the divided part, I discovered another stricture nearly an inch higher up, of the same character, indurated, and irregularly thickened. The knife was passed on in the same careful manner, and the part divided. The fore-finger now could be passed with the greatest facility through the whole of the diseased part, being in extent rather more than one inch. Some little bleeding followed the incisions, so that it was necessary to introduce a large rectum bougie, with lint upon it, through the divided part. The hemorrhage, however, soon stopped, and not more than an ounce of blood was lost altogether. In two hours from the operation the bowels began to act, and an immense discharge of fæces of a very offensive character took place. This discharge of excrement continued for two or three days, until the bowels were thoroughly emptied. The symptoms, however, of pain of the abdomen, diarrhœa, &c. continued for some time. These were combated by the usual methods of treatment; and on December 7th the patient was discharged perfectly well;—the passage of the rectum being completely open, and the fæces passing naturally, and with freedom.

Art. VI.—*Observations on the Blood after Death.* By John Davy, M. D. F. R. S., Assistant-Inspector of Army Hospitals.

For many years past, in the *post mortem* examination of bodies, I have been in the habit of paying attention to the condition of the blood in the heart and great vessels, especially in relation to coagulation;—and latterly, in conjunction with coagulation, to the effect of agitation on the blood, in common air,—as a test whether or no it contains carbonic acid gas, in excess, admitting of being extricated by this method.

I shall first give the general results of my observations made in Malta from the year 1828 to 1835,—as an example of the variations in the appearances of the blood after death, and of their comparative frequency. For the sake of brevity I shall present them in a tabular form.

TABLE I.—Of the Appearances of the Blood after Death from various Diseases.

Year.	Total number of fatal cases examined.	Heart collapsed, contain. either no blood, or very minute quan.	Blood liquid, and not coagulating on exposure to the air.	Partly liq. partly coagulat. former not coagulating on exposure.	Partly liq. partly coagulat. former coagulating on exposure.	Liquid and coagulating on exposure.	Coagulated and containing fibrinous concretions.	Coagulat. without fibrinous concretions, the coagulum soft.	Grumous, not distinct. coagulat. without fibrinous concret.	Liquid and frothy, without any fibrinous concretions.	Blood florid.	Fibrinous concretions without any cruor.	Blood coagulated in both ventricles and broken up.	Coagulated and broken up in left ventricle, not in right.	Coagulated in right ventricle, and broken up; left ventr. empty.	Cases in which the state of the blood was not noticed.
1828,	28		2	2	2	1	21									
1829,	18	2			2		14									
1830,	36		1	1	1		18		2	1	1			1		11
1831,	54	2	1				21	3					3	5		19
1832,	29	2	2				8	1					1		1	14
1833,	35		2			1	7	3					1	4		17
1834,	40		1		1	2	14	3				1		1		17
1835,	9						2									7
Total,	249	6	9	3	6	4	105	10	2	1	1	1	5	11	1	85

The fatal cases, in which these observations were made, occurred, without exception, in soldiers of our regiments serving in Malta, composed of Englishmen, Scotch, and Irish.

In the cool season, the bodies were commonly inspected in from twelve to seventeen hours after death; in the hot season, in from three to twelve.

As the table is brought forward, chiefly as an example, intended to show how various is the condition of the blood after death, I shall restrict myself to a very few remarks on the different appearances of the blood noticed, and especially in connection with the fatal diseases. To do justice to the subject, it would be necessary to enter into very minute details, and give a statement of particulars, which could be collected only by laborious research,—and which at present I am not prepared to offer.

1. The few instances in which the heart was found empty, or containing only a very minute quantity of blood, were chiefly cases of death from ruptured aneurism.

2. In the instances in which the blood was found liquid and not coagulable on exposure to the air, the fatal event, in the majority of cases, was referable to the respiratory organs, and was owing either to drowning, hanging, the fumes of a charcoal fire, or to effusion of blood either into the bronchia, or into the air-cells. The former occurred in a case of pneumonia; the latter in two cases of sudden death, from what Laennec has called pulmonary apoplexy. The blood's liquidity in these instances was not a *post mortem* effect resulting from putrefaction; this was clearly ascertained. It was owing either to a loss of the natural fibrin or lymph on which the property of coagulation depends, or to such an alteration in it, as to deprive the lymph of this quality. In one instance, a few days previous to death, some blood abstracted from the patient, exhibited, in relation to coagulability, its normal condition.

3. Relative to the condition of the blood, noticed in the fourth and fifth columns, as either liquid and coagulating on exposure, or partly liquid and coagulable,—I believe it depended, whether one or the other, on the time after death, when the *post mortem* examination was made. I shall have occasion farther on, to bring forward examples in illustration of this remark.

4. Of the 164 cases in which the blood was specially noticed, in 105 instances, it was found coagulated, the coagulated blood mixed with fibrinous concretions or coagulated lymph, the material of the buffy coat; and I believe it was found in the same state in the majority of the 85 cases, in which, from no note having been taken at the time, its condition was not specified.

5. The instances in which the blood was found coagulated and without fibrinous concretions, (the coagula soft,) were chiefly cases

of tubercular phthisis; in the majority of the fatal cases, however, of this disease, fibrinous concretions occurred.

6. In one instance, in which the blood was found grumous, not distinctly coagulated, death was owing to confluent variola; the lungs were hepatized. In the other instance the disease was returned "debility,"—was of a very obscure nature, and not connected with any well-marked organic lesion. Whilst grumous blood was found in the cavities of the heart in this case, fibrinous concretions were met with in the great vessels.

7. In the solitary case, in which the blood was found liquid and frothy, and not coagulating on exposure to the air, putrefaction was far advanced, and it is probable, that the fibrin had become liquid after coagulation, in consequence of putrefaction. In this instance, all the parts, in contact with blood, were strongly stained by the colouring matter of the blood in a state of solution in the serum. The phenomenon of the red stain, as a *post mortem* effect, is, I believe, invariably connected with this condition of the serum, and which, itself, is connected more or less with putrefaction. I have never yet met with an exception to this remark. Whenever I have found red serum, and have subjected the blood to microscopical examination, I have detected change in the form of the red particles, indications more or less of decomposition.

8. The single case in which fibrinous concretion occurred in the heart without crassamentum or cruor was one of dysentery.

9. The single case in which the blood was observed to be fluid, was an instance of *tabes mesenterica* without tubercles in the lungs. The mesenteric and lumbar glands were vastly enlarged; some of them the size of pigeon's eggs, and contained a curd-like albuminous matter; and tubercles containing a similar matter were found under the mucous coat of the transverse colon. The blood in this instance was coagulated;—its florid hue was most strongly marked in the aorta.

10. The broken up state of the coagulated blood, referred to in the three last columns of the table, to the best of my knowledge, has not yet been described by any pathological writer. The appearance exhibited was as if one or both ventricles of the heart had continued to act after the coagulation of the blood had taken place within them. On no other idea can I account for the phenomenon. I never witnessed it in the blood-vessels, or even in the auricles; which perhaps may be considered in accordance with the supposition respecting its cause. Of the seventeen instances in which this condition of the blood was detected, death in the majority was owing to tubercular phthisis, viz. in eleven; in two the fatal disease was pneumonia; in one abscess of lung, accompanied with gangrene; in one chronic dysentery, accompanied with abscess in the liver; and, in one, the death was from violence; a soldier

in good health was shot through the head by a comrade. In the majority of instances, fibrinous concretions accompanied the broken up crassamentum.

I shall now proceed and briefly notice some later observations on the blood after death, made in the General Hospital, at Fort Pitt, during the last nine months, in which, attention, as before-mentioned, was paid, not only to the state of the blood in relation to coagulation, but also to its state in relation to carbonic acid gas.

The method employed, was briefly, the following. In every instance, after the removal of any fluid that might be found in the pericardium, the great vessels within it were divided; the blood which flowed out was collected and put into a bottle and secured with a glass-stopper, for subsequent observation and experiment. The cavities of the heart and their contents were next examined. Sometimes, the blood set apart, was seen again the same day, to ascertain, whether it had coagulated, or remained liquid; but more frequently, not until the following morning, when also commonly it was subjected to agitation in atmospheric air, included in a double-mouthed bottle, provided with stop-cocks, to one of which was attached a bent tube for communication with a pneumatic trough.

I shall give the results in a tabular form, with the age and fatal disease, individually,—the day of the month of its termination,—the interval between the time of death and of the *post mortem* examination,—and between the latter and the examination of the blood,—circumstances, without a knowledge of which, little value can be attached to the record. Under the head of Remarks, a notice will be appended of the organic lesions discovered of most importance, characteristic of the disease, with occasionally those of rare occurrence.

Further, I may premise, that in using for the sake of brevity, the term cruor, I have not employed it to designate merely a mixture of serum and red particles, but a mixture of the two in which there may be some coagulable lymph to be determined by trial. The terms crassamentum and clot I have used synonymously, in their ordinary acceptation, as applied to the blood. The term fibrinous concretion I have employed to designate masses of lymph or fibrin, in a coagulated state, such as used to be called polypi.

Table II.—Of the condition of the Blood after Death from various Diseases and its changes.

No.	Age.	Fatal disease determined by dissection.	Time of death.	Inspect. hours after death.	General condition of the blood in the heart.	After condition of the blood set apart.	Result of the agitation of the blood in cruor.	Remarks relative to morbid appearances.
1	19	Pericarditis	Jan. 20	16	Pretty much cruor, & a little clot in right cavits. no fibrinous concretions		Agitated, immediate. absorbed a little air, and again the following day.	Pericardium nearly an inch thick, from deposition of lymph; contained 20 ounces of purulent serum; granular tubercles in the lungs.
2	29	Phthisis pulmonalis	26	18	About 3 oz. of cruor, and dark crassamentum in right cavities.	Two hours after, the cruor feebly coagulated.	Immed. after coag. gave off some air on agitat. On 1 Feb. had an offensive smell, & absor. a little air.	Numerous tubercles and vomicæ in the legs; 46 oz. of purulent serum in left pleura; it gave off a good deal of air on agitation.
3	22	Rubeola.	Feb. 3	28	Much dark liquid blood in the right cavities		About 2 h. after, gave off some air; a 2d time immed. a slight absor.; rub. with quicklime a slight ammoniacal odour.	Bronchia dark-red; engorgement of pulmonary vessels; some ecchymosis in air-cells. Death on fourth day.
4	18	Phthisis pulmonalis.	19	29	Much cruor, with fibrinous concretions in right cavities	Six oz. of cruor collected; liquid 20 hours after.	20 hours after, gave off about half a cubic inch of air.	Lungs abounded in tubercles and small vomicæ; partially hepatized; partially œdematous; a portion of large vein in right lung, plugged up with firm fibrin.
5	19	Bronchitis.	23	33	Much cruor and concretions in right cavs.; former of sp. grav. 1045	Cruor did not coagulate.	Agitated on 28 Feb. gave off no air; 3 ds. later gave off some; smell putrid.	Bronchia smeared with pus; an ulcer on upper surface of epiglottis; two small clusters of granular tubercles in lower part of superior lobe of right lung.
6	20	Phthisis pulmonalis.	Mar. 22	30	A good deal of cruor and concretions in right cavs.	6 oz. of cruor collect.; on following day slightly coagul.	On agit. after hav. been broken up, a very slight absorp.; agit. with carb. acid, absor. 116 per cent.	Cavities and tubercles in both lungs.
7	24	Chronic peritonitis.	30	14	Some cruor, soft crassamentum, and a little fibrinous concretion in right cavities.	Next morning, the cruor slightly coagulated.	Pretty much air disengaged, and also from serum.	7 lbs. serum in peritoneum; thickening of peritoneal covering of most of the viscera; a few clustered granular tubercles in lungs; tubercles in kidneys, prostate and testes; loose, putty-like albuminous matter in left vesiculæ seminales.

No.	Age	Fatal disease determined by dissection.	Time of death.	Inspect. hours after death.	General condition of the blood in the heart.	After condition of the blood set apart.	Result of the agitation of the blood in cruor.	Remarks relative to morbid appearances.
8	70	Chronic peritonitis; pulmonary abscess.	Ap. 13	48	Only about 3 oz. of blood in the heart (clot) without fibrinous concretions		Clot broken up and agitated, a slight absorption.	A small gangrenous abscess in right lung; no tubercles in either lung; 16 pints of serum in cavity of abdomen; some pus in hollow of pelvis; liver only 2 lbs.; liver and pancreas unusually firm. The serum agitated following day, gave off a good deal of air. The patient for many years had laboured under amentia.
9	59		May 3	26	Cruor and soft coagula in right cavities.	Next morning cruor fully coagulated.	Pretty much air disengaged; agitated after having been broken up.	Blood effused on left hemisphere of cerebrum on the arachnoid membrane; fornix soft. Inferior lobe of left lung œdematous; two cavities in it containing a puriform fluid; no tubercles. Had long laboured under amentia.
10	20	Phthisis pulmonalis.	5	22	Pretty much cruor and fibrinous concretions in the cavities generally.	Cruor next morning slightly coagulated.	Broken up; a good deal of air given off in agitation.	Many clustered granular tubercles, and two or three small cavities in superior lobe of left lung; several similar clusters in superior lobe of right lung; right lung extensively hepatized; 8 oz. of purulent serum in right pleura; 3 oz. in left; 2 oz. in pericardium.
11	21	Œdema glottidis.	6	31	A good deal of dark liquid blood in large vessels, and in right cavities of heart; a small portion of fibrin in right ventricle.	After 3 hours continued liquid.	On agitation gave off a good deal of air.	Unexpected and almost sudden death from œdema of glottis, margin of epiglottis, and sacculi laryngis; the uvula œdematous.
12	30	Bronchitis; Chronic phthisis pulmonalis	17	48	A good deal of cruor and fibrinous concretion in cavities of heart.		On agitation immediately gave off some air.	Bronchia red; contained muco-purulent fluid; lower surface of epiglottis superficially ulcerated; an ulcer at base of right arytenoid cartilage; numerous granular tubercles in both lungs; a small vomica in superior lobe of right; three in the superior lobe of left. The body little emaciated.
13	29	Phthisis pulmonalis.	17	45	Some cruor and fibrinous concretion in right cavities.		On agitation immediately gave off some air.	Lungs abounded in tubercles, and contained several cavities; large intestines severely ulcerated. Died suddenly in the act of speaking.
14	31	Phthisis pulmonalis.	25	27	A good deal of coagulated blood and cruor and fibrinous concretion in right cavities of heart.	Cruor next morning liquid.	Gave off a little air.	Large cavities and numerous tubercles in the lungs; lungs partially hepatized. In right ventricle of heart, two masses of fibrin closely adhering, in each of which a puriloid fluid, consisting (as it appeared under the microscope) of a few blood-disks,—globules very like those of pus,—and of other particles less regular, and of various sizes.

15	31	Pneumonia.	May 26	26	A good deal of cruor, clot, and fibrinous concretion in cavities of heart.	Cruor liquid next morning.	Gave off some air.	Some œdema of left lung, weight ¾ lb.; right lung extensively hepatized, of a fawn-colour, its weight 4 lbs. Slight deposition of lithate of soda on cartilages of tibia, and of first joint of great toe. Reported to have been subject to rheumatism.
16	28	Phthisis pulmonalis.	June 1	12	Some clot, fibrinous concretions, and a good deal of cruor in right cavities	Two hours after cruor jellied.	After 24 hours, coag. had contracted, serum separated; broken up and agitated, gave off some air.	Cavities and numerous tubercles in lungs; severe ulceration of larynx. In a mass of fibrin in right ventricle, a collection of transparent serum; the mass was firmest externally.
17	34	Aneurism.	July 21	26	Six ounces of thin cruor in right pleura, with a very small portion of clot.	The cruor did not coagulate.	Agitated 24 hours after, gave off a very little air.	A false aneurism close to cœliac artery, with two circular openings from aorta within half an inch of each other; the sac having ruptured, a large quantity of blood was poured out and coagulated in omentum; a portion entered the posterior mediastinum, and ruptured it.
18	30	Phthisis pulmonalis.	24	26	A good deal of dark cruor and fibrinous concretions in right cavities; a mass of fibrin in left ventricle.	The cruor (6 ounce) was found jellied the following day.	Broken up and agitated, it gave off a little air.	Several cavities in lungs, and numerous tubercles, with some hepatization.
19	30	Phthisis pulmonalis.	29	27	Some cruor and fibrinous concretions in right cavities.	Cruor after 24 hours slightly jellied.	Broken up and agitated, gave off a very little air.	Tubercles abounded in lungs, with cavities, partial œdema, and partial hepatization.
20	24	Phthisis pulmonalis.	31	37	Some cruor, clot, and concretions of fibrin in right cavities, and in left auricle.		Agitated immediately, gave off pretty much air.	Right lung weighed 3½ lbs., partially hepatized; contained cavities, and abounded in tubercles; left lung similarly diseased in a minor degree.
21		Hypertrophy of heart.	Aug. 1	32	A good deal of cruor and some clot in right cavities.		Agitated immediately gave off a good deal of air.	Heart nearly triple its natural size; ventricles most enlarged, especially left. Aorta much dilated and diseased; an aneurismal pouch at its base. Albuminous matter of the appearance and consistence of putty in left *tunica vaginalis testis*,—left *vesicula seminalis*, in substance of kidney, in cavities.—in ureter of the same side,—and in right *vas deferens*. The left ureter was in several places impervious. Partial œdema of lungs; no tubercles.
22	3[illegible]	Phthisis pulmonalis.	5	4	Much liquid blood in cavities of heart, some clot, no fibrinous concretions.	63 ounces collected, which coagulated in a few minutes.	Broken up the following day, gave off pretty much air.	Large excavations and numerous tubercles in lungs. Death probably from suffocation,—from the bursting of a vomica, and the pouring its contents into the bronchia.

No.	Age.	Fatal disease determined by dissection.	Time of death.	Inspect. hours after death.	General condition of the blood after death.	After condition of the blood set apart.	Results of the agitation of the blood in cruor.	Remarks relative to morbid appearances.
23	46	Hydrothorax.	Aug. 4	29	A little cruor and clot in right cavities.	Only about 1 oz. of cruor could be collected; next day was found coagulated.	Quantity too small for trial.	54 oz. of serum in right pleura, with flakes of lymph; 18 oz. in left pleura; 5 oz. in pericardium, without deposition of lymph: the serum agitated, gave off pretty much air.
24	22	Phthisis pulmonalis.	9	12	Pretty much cruor and fibrinous concretions in right cavities; very little in left.	Cruor did not coagulate.	The following day gave off pretty much air.	Granular tubercles in lungs; 32 oz. of serum in cavity of abdomen. Had all the blood in the body been collected, it would not have exceeded, it is my belief, 4 ounces. 27 oz. of seropurulent fluid in left pleura; pleura lined with false membrane. Cavities and tubercles in the lungs. A mass of softened fibrin in right ventricle, which, it may be inferred, existed before death: a mass of firm fibrin in left ventricle of heart.
25	25	Lumbar abscess.	22	14	A good deal of cruor, and a little soft fibrinous concretion in right cavities.	Cruor found coagulated next morning.	Broken up and agitated, gave off pretty much air.	Pus in bronchia of left lung; a portion of its inferior lobe hepatized; another portion œdematous. No tubercles. Lumbar vertebræ and right *os ilii* extensively carious.
26	28	Pneumonia.	5	19	A good deal of cruor, clot, and fibrinous concretions in the heart.	Two hours after removal, cruor found coagulated, and covered with a thin buffy coat.	Broken up and agitated; the following morning gave off no air, and did not absorb any.	Right lung weighed 3½ lbs.; its superior lobe completely hepatized; some of its vessels filled with lymph; left lung in part œdematous; no tubercles. Liver very large, not fatty, weighed 13½ lbs.; 8 oz. of serum of sp. gr. 1010, without any lymph in the cavity of the abdomen.
27	47	Phthisis pulmonalis.	14	24	Some cruor, a good deal of clot and fibrinous concretions in right cavities; some fibrinous concretion in left ventricle.	Cruor the next day found jellied.	Broken up and agitated, gave off a little air.	Both lungs contained numerous tubercles and some cavities. The arch of aorta considerably enlarged; two masses of fibrin adhered to it internally, a little below the mouth of the *A. innominata*, about the size of filberts undergoing softening.
28	30	Medullary tumour	17	5	Some cruor and a good deal of clot in right cavities.	Cruor next morning found firmly coagulated.	Broken up and agitated, it gave off a little air.	A considerable portion of left side of face destroyed, with greater part of corresponding maxillary bones; a tumour about the size of a large lemon over the left parotid gland, its central point, soft, reddish, and blood-shot, composed of particles, which under the microscope appeared of different sizes; some of them globular. Viscera tolerably sound.

29	40	Dysenteria chronica	August 18	4½	Some cruor and fibrinous concretions in right cavities of heart.	After 2 hours the cruor found coagulated, and a slight buffy coat on it.	Broken up and agitated the following day; no air given off; a very slight absorption.	Large intestines severely ulcerated. A mass of fibrin in longitudinal sinus, softening internally.
30	26	Phthisis pulmonalis	19	16	Some cruor in right cavities; no clot; no fibrinous concretion.	Cruor liquid the following day.	Gave off no air; no appearance of absorption.	Vomicæ and numerous tubercles in lungs. *Appendicula vermiformis* distended with purulent fluid; ulcerated; its orifice perfectly closed by the adhesion of its sides.
31	22	Phthisis pulmonalis	19	18	Some cruor and fibrinous concretions in right cavities of heart.	Cruor next morning slightly jellied.	Agitated, gave off a good deal of air.	Numerous tubercles and many minute vomicæ in lungs; a portion of right ulna in a state of necrosis; cartilage of head of humerus and of glenoid cavity destroyed; pus in the capsule.
32	40	Phthisis pulmonalis	22	8½	Some cruor, much clot, and some fibrinous concretions in right cavities.	The cruor after 24 hours found jellied.	It gave off little air.	Left lung very small from compressions. Many tubercles in it, and a cavity communicating with the pleura, which contained 57 oz. of sero-purulent fluid and a good deal of air. Vomicæ and tubercles in right lung. The fluid from pleura on agitation gave off a good deal of air.
33	41	Phthisis pulmonalis	26	5	Some cruor and fibrinous concretion, and a good deal of clot in right cavities of heart; little in left.	The cruor next day found coagulated.	Broken up, it gave off no air, and did not appear to absorb any.	Vomicæ and tubercles in both lungs. Left femoral and iliac veins with their branches obstructed by coagulated blood and fibrinous concretions; the latter in many places softening, and in one or two reduced to a puriloid state.
34	20	Ascites	28	27	No blood in heart; blood from jugular vein liquid.	The following day liquid.	On agitation, it gave off pretty much air.	7 pints of turbid serum in right pleura; 84 oz. of clear serum in cavity of abdomen. Some pus in pericardium, which greatly thickened and partially adhering. A few crude tubercles in lungs and one small cavity. The serum from abdomen was of sp. grav. 1008; the following day a little lymph was found deposited from it; it gave off a good deal of air on agitation.
35	45	Medullary tumour	Sep. 1	21	A good deal of soft clot; some fibrinous concretions, and a very little cruor in heart.	The cruor did not coagulate.	The quantity too small for trial for air.	Masses of cerebriform tumour in left arm, accompanied with much disorganization of soft parts. Similar disease in substance of left kidney. The lower part of *vena cava*, the left iliac and femoral veins and their branches obstructed by coagulated blood and fibrinous concretions, the latter in many places softened and puriform.

The instances in the first table were strongly indicative of the variable quality of the blood after death from disease; and the particular instances just given are as strongly corroborative of the same. The variable quality, as far as appreciable by the senses, seems to depend on the lymph, which, as regards coagulability, has appeared wonderfully little constant; in some instances retaining its power of coagulation more than twenty-six hours after death; in others coagulating almost immediately after the fatal event; and in some previous to that event. In a large number of cases, the lymph appears to have existed in the blood, possessed of the quality in question in different degrees. This is clearly to be inferred from the results of the *post mortem* inspections, namely, from finding at the same time and in the same body, in the cavities of the heart,—liquid coagulable cruor, soft crassamentum, and firm fibrinous concretions.

The fibrinous concretions which are so frequently found in the heart after death, are far from being devoid of interest. They too are very various, both as regards form and consistence. They are also very various in relation to the manner in which they are attached to the heart. I have mentioned one instance, in which serum was collected in the interior of a mass; the inner part was soft, the outer surface firm. Some masses seem to have a certain regularity of structure. Some have the appearance of being inflamed, being blood-shot from entangled red particles of blood. Some exhibit the appearance of abscess, and contain as it were in a sac, a puriform matter, generated by a peculiar process of softening. Generally, they tend to give the idea of the operation of an organizing principle, a *nisus formativus*, more or less active, until putrefaction commence, by which the animal matter is converted into common matter.

The different varieties of fibrinous concretions are admirably illustrative of certain morbid appearances, especially false membranes, bands of adhesion, the state of the cellular tissue in different kinds of œdema and induration, and, I may add, hepatization of the lung with and without softening, and a similar state of the spleen, and perhaps tubercles and their softening. When I have examined, microscopically, the matter yielded by the lung which has been hepatized, undergoing softening, I have found it very similar to the matter into which a fibrinous concretion is converted, whether in the ventricles of the heart or in a vein, composed of particles of different sizes, most of them approaching the spherical in form, and some of them very like pus globules; and, I have found the same kind of matter in softened spleen, and also in softened tubercular matter.

No subject, probably, in pathology is more deserving of careful investigation than lymph, in connection with its varieties, supposing either that there are many different species of it, or that it is liable to sudden and great changes of quality. Relative to the former, take for instance the phlebolite, in its different stages of induration, from soft lymph becoming of cartilaginous firmness and appearance; the fibrinous layers deposited in an aneurismal sac, resisting changes, occasionally for years; and the fibrinous concretions which are formed in the large veins, especially in phthisis, which so soon soften and in part almost liquefy,—how great is the diversity, how difficult in the present state of our knowledge the explanation.

Nor is the liquid cruor or the blood remaining either in part, or altogether liquid after death, whether for a definite or indefinite time, without interest and importance, especially as regards pathological research and the distinguishing between the effects of disease and *post mortem* effects, in the inspection of bodies. So long as the blood is liquid after death, it must necessarily observe the laws to which fluids are subject under similar circumstances; will accumulate in the dependent parts and where there is least resistance; and, should there be considerable pressure exerted, as from the disengagement of air in the stomach and intestines, it will appear as if injected in the organs removed from such pressure, whether included within unyielding parietes, as the brain, subject to occupy less volume from greater reduction of temperature after death than its hard, and, during life, cooler case; or contained in yielding envelopes, as the lungs in the pleuræ and other parts of the body generally, in the common integuments and in their peculiar capsules. In a considerable number of examples given in the table, cruor was found in the heart more than twenty-four hours after death. This cruor in producing false appearances of inflammation and congestion would act much in the same manner as liquid blood; and, in estimating the phenomena which are presented on dissection, much the same allowance should be made for it, or serious errors will be unavoidable.

From the experiments given in the table on the agitation in air of the blood collected after death, it would appear, that in the greater number of instances, gas was disengaged. In several instances, the air thus liberated was tried, and was found to be carbonic acid gas; proving that there was an excess of this acid in the blood. This excess, I am disposed to infer, especially from the results of experiments which I have recently made on healthy blood, an account of which is published in the Philosophical Transactions for the present year, is the effect of disease, and is particularly apt to take place in the act of dying, when the powers of secretion seem to be arrested, and when carbonic acid probably ceases to be eliminated in the lungs. In accordance with this, with very few exceptions, is the uniform dark colour of the blood usually observed after death, and equally in the left and in the right ventricle of the heart; and also, the little suffering commonly witnessed in dying,—diminished sensation probably as in birth, or stupor, or coma, usually preceding and ushering in the fatal event.

From the results referred to, and from a few which I have obtained in operating on blood taken from persons labouring under disease, I am disposed to think that carbonic acid acts a very important part in the economy of life; and is connected when in excess, if not with the production of particular diseases, at least with their modification and progress and the production of certain symptoms, and that the careful examination of the blood, in relation to this acid, is an inquiry, much wanted, and is likely to reward richly those who engage in it. At present, there is a great difference of opinion derived from experiment, respecting the carbonic acid in the blood;—some inquirers, as Dr Stevens, to whom belongs the merit of having opened this path of research, considering it, in the healthiest condition, always in excess and capable of being liberated from venous blood and even from arterial, by the physical agencies in play in respiration; whilst others take an opposite view, and deny that any free acid exists in the blood in its healthy normal state, or can be separated from it excepting by processes in which it is formed from its elements. This discrepancy, the inquiry I have mentioned, will probably explain or reconcile. It will elucidate also, probably, some other obscure points, which might be mentioned, especially were it extended to other fluids, as the urine and bile. I may mention, incidentally, that whenever I have obtained indications of the disengagement of carbonic acid from the blood after death, they have been afforded by the fluids spoken of, in a very limited number of trials, and also by any effused serous fluid collected at the same time, and subjected to the same test.

I may add, that in most of the cases brought forward in the second table, the blood was microscopically examined. I employed for this purpose a very good instrument, constructed by that excellent maker Mr Ross, provided with an achromatic object-glass of the one-eighth of an inch focal distance. I shall notice very briefly some of the results.

The blood corpuscles were seldom found perfectly uniform either in size or appearance. In some instances they appeared thinner and more expanded; in some, of perfectly regular outline, as in their normal state; in others, the discs had an uneven and indented margin, and a puckered surface; in some, in the same specimen, the particles exhibited these variations.

In most cases, in which gas was disengaged from the blood on agitation in common air, the blood-discs, under the microscope, appeared to have minute globules attached to them; and, very often instead of gliding on their flat surface, or rolling with a revolving motion, they moved in the current passing under the object-glass, in a perpendicular position, as if supported in this manner by particles of air adhering to their edge,

In every instance, in which, during life, there were purulent discharges, or purulent collections were detected after death, globules like pus-globules were visible under the microscope on dilution of the blood with water, and they commonly appeared intermixed with very much smaller globules and particles more or less irregular in form. For the purpose of examination, occasionally a portion of a drop of blood was used;—occasionally several ounces;—in the one instance it was mixed with water and immediately subjected to the microscope;—in the other the sediment was so subjected, obtained by successive washings and rest,—repeated until all the colouring matter was removed. I was led to institute the trials, from some observations of Mr Gulliver, who, I believe, was the first to detect pus in the blood, and to appreciate its pathological influence. The observations I refer to, are contained in a very original and ingenious paper by this gentleman, which was read before the Royal Society last spring, and which has since been published under the title of "Researches on Suppuration," in the Philosophical Magazine for September.

This discovery, which I attribute to him, of course rests on precise microscopical observation, and is not to be confounded with the supposed detection of pus in the veins, and its inferred existence in the blood in instances designated phlebitis, the majority of which hitherto described, I am disposed to believe, were examples not of true inflammation of vein, but merely of obstruction from the formation of a fibrinous coagulum with a tendency to soften and assume a puriloid character.

Fort Pitt, November 1, 1838.

ART. VII.—*An Historical Account of Uterine Hemorrhage in the latter Months of Pregnancy.* By ROBERT LEE M. D., F. R. S, Physician to the British Lying-in Hospital, and Lecturer on Midwifery at St George's Hospital.

BEFORE the commencement of the seventeenth century, few facts of importance had been ascertained respecting the causes and treatment of uterine hemorrhage in the latter months of pregnancy. Guillemeau then stated, that the placenta sometimes presents, or comes before the child, and that this gives rise to a dangerous hemorrhage, which nature is unable to suppress. He observes in Chapter xii. which is entitled, "Le moyen de secourir la femme quand l'arriere-faix se presente le premier," that when the afterbirth comes first, there is a flooding, and that the most safe and expedient means of arresting the hemorrhage is to deliver immediately, by passing up the hand into the uterus, and turning the child. He has made no observation, from which it can be inferred that he believed the placenta to have been originally adherent to the upper part of the uterus, and to have descended from thence to the cervix. From the same word " bouchée" being afterwards employed by Paul Portal, when he is relating cases in which the placenta adhered from the first to the neck of the uterus, it is probable Guillemeau was also of opinion, that it was originally attached to the inferior segment of the uterus.

In those cases of uterine hemorrhage in which the placenta did not present, but had been detached by some accidental cause from the *fundus uteri*, Guillemeau had likewise recourse to artificial delivery, and for the knowledge of this practice he states that he was indebted to Ambrose Paré. The same plan of treatment was therefore employed by him in all the varieties of uterine hemorrhage in the latter months of pregnancy.

The xxviiith chapter of Mauriceau's treatise (1668) is entitled " De l'accouchement auquel l'arriere-faix se presente le premier ou est tout-a-fait sortit devant l'enfant." The symptoms and treatment of cases of placental presentation are here accurately described, and in all cases of hemorrhage from this cause, he recommends immediate delivery. If the placenta has not entirely escaped, and the membranes are not ruptured, he advises the part of the placenta which presents to be put aside with the hand, which is to be passed up into the uterus, the membranes ruptured, and the delivery completed by turning. The rules for the treatment of these cases are laid down with the greatest precision. When the placenta was entirely separated, then only did he consider it as a foreign body, and recommend its extraction before the child; but to this practice he states as an obvious objection, that the placenta is strongly attached to the membranes which surround it, and that it cannot be drawn out without the membranes enveloping the body of the child being drawn out also. Mauriceau has related seventeen cases of uterine hemorrhage in the latter months of pregnancy from presentation of the placenta, and in sixteen of these delivery was accomplished artificially by passing the hand through the opening formed by the separation of the placenta from the uterus, rupturing the membranes and turning the child. Two women died after this operation, and one who would not consent to have it performed died undelivered.

In the history of case 423, related at p. 350, the following observation occurs, and may be adduced as a proof that Mauriceau was aware of the fact, that the placenta had not been wholly detached from the uterus. " Mais quoique j'aye dit que l'arriere-faix de cette femme presentoit le premier au passage, dans le temps que je l'accouchai et que l'excessive perte de sang qu'elle avoit,

vint de ce detachement, il ne faut pas croire que cet arriere-faix fut ainsi entierement detaché de la matrice, depuis tout le temps que cette perte de sang avoit commencé à parôitre en cette femme: car si cela eut été, l'enfant seroit mort en tres peu de temps, ne pouvant pas être vivifié que par la communication du sang de la mere, dont il est privé aussitot que l'arriere-faix est entierement detaché de la matrice: mais comme il n'y avoit dans le commencement de cette perte de sang que quelque petite partie de l'arriere-faix qui s'en etoit un peu detachée, cela n'avoit pas empeché l'enfant d'etre nourri du sang de tout le reste de l'arriere-faix qui n'avoit pas eté entierement separé de la matrice."

A Tabular View of Seventeen Cases of Placental Presentation recorded by Mauriceau.

No. of Case.	Date. A. D.	History and Treatment.	Result.
8	1669.	Placenta partially expelled; a foot and knee presenting. The placenta pressed back and the child extracted.	Recovered.
55	1672.	Hemorrhage in the seventh month; faintness; delivery by turning the child. He states that he had successfully delivered many women with flooding in the same manner.	Do.
59	1672.	Flooding for six hours at the sixth and a-half month of pregnancy; frequent syncope; *os uteri* gently dilated. Head of the fœtus and placenta pushed aside, and the feet brought down; masses of coagula afterwards expelled. Child alive.	Do.
68	1672.	Flooding for a month; labour pains; child turned: cord twice around the neck. Child alive.	Do.
106	1674.	Seventh month. Entire ovum extracted or expelled.	Do.
170	1676.	Seventh month; *os uteri* hard, thick, and little dilated; she would not consent to any interference, and died undelivered.	Died.
175	1676.	Flooding in the eighth month, preceded by a fall. Delivered by turning. The child dead.	Recov.
210	1678.	Seventh month; great flooding; prolapsus of funis. Placenta pushed aside by the hand, and the child turned.	Do.
	1678.	The circumstances similar; and delivery by turning the child performed.	Do.
423	1686.	Flooding at the eighth and a-half month, of four weeks duration. Delivery by turning the child. He states that the placenta was not entirely separate from the uterus.	Do.
428	1686.	Dangerous flooding in the ninth month; of four weeks continuance. Immediate delivery by turning the child.	Do.
438	1686.	Flooding and convulsions; *os uteri* thick and hard. Death two hours after delivery.	Died.
454	1686.	Flooding in the seventh month. Placenta presenting as in all the former cases.	Recov.
484	1687.	Great hemorrhage at the eighth and a-half month. Placenta presenting and entirely detached. Delivery by turning. Died twelve days after from diarrhœa.	Died.

No. of Case.	Date. A. D.	History and Treatment.	Result.
502	1687.	Eighth month. Placenta entirely separated from the uterus; delivery by turning; great headach followed.	Recov.
597	1690.	Great flooding in the seventh month, caused by detachment of the placenta, which presented; arm presentation; turning; child alive.	Do.
651	1692.	Flooding in the sixth month, caused by the entire detachment of the placenta; frequent syncope; delivery by turning; child dead. He remarks that we must never trust to nature in such cases, for without assistance the child cannot be expelled, and that the flooding will not cease till the uterus is emptied.	Do.

Mauriceau has also recorded the histories of thirty-seven cases of uterine hemorrhage in which the placenta did not present, but had adhered to the upper part of the uterus, and been accidentally detached. Twenty-one of these cases occurred before the year 1682, and in almost all of them he delivered artificially, by passing the hand into the uterus, rupturing the membranes, and turning the child. The treatment he employed in these cases was therefore precisely the same as that which he had employed in the sixteen cases of placental presentation, and differed in no respect from that recommended by Guillemeau. But in the year 1682 his case, No. 307, occurred, and in the treatment of this, he deviated from the former practice, and, instead of delivering immediately by turning the child, he ruptured the membranes, and left the labour to nature with the happiest result. "On the 9th of June 1682," he says, "I delivered a young woman in the eighth month of pregnancy, who had uterine hemorrhage caused by a violent fall upon the knees four days before. During her whole labour she had only slight pains in the abdomen, which produced no effect. As the hemorrhage was moderate, and the uterus was gradually dilating, I committed the labour to nature, contenting myself with rupturing the membranes of the child. "Afin que dans l'impulsion des douleurs, les membranes qui sont attachées dans toute la circonference de l'arriere-faix, ne le fissent pas detacher davantage de la matrice, et n'augmentassent pas pour ce sujet, la perte de sang avant la sortie de l'enfant, qui fut heureusement poussé dehors une petite demi-heure apres que j'eus ainsi rompu les membranes des eaux."

Mauriceau gives no account of the circumstances which induced him to make this important change in the treatment of cases in which the placenta did not present, and to adopt that method of treatment which was at a much later period so strongly recommended by Puzos, and considered by him as his own discovery. From a case of hemorrhage arising from detachment of the placenta from the upper part of the uterus, which occurred in 1685, and another in July 1686, where he had recourse to the operation of turning, it is probable Mauriceau was not convinced of its safety before the month of August 1686. His 450th case then occurred, and the pains being feeble and the hemorrhage great, he ruptured the membranes, by which means the infant could advance into the passage, without pushing the membranes before it, and further detaching the placenta, and increasing the hemorrhage as it had previously done. The *liquor amnii* having escaped, after the rupture of the membranes, the labour-pains, which before had been feeble, speedily became strong and effective, and the patient was soon delivered of a living child, and recovered favourably. Case 429 occurred in 1687, and in this also he ruptured the membranes, which he observes should be done in all similar cases. Cases 480 and 496 occurred also in 1687, and the membranes were ruptured in both, and the labour left afterwards to nature. The grounds upon which he had recourse to this practice are again forcibly stated in the history of the last of these cases. Case 542 took place in 1688. The patient was in the seventh month of pregnancy, the feet presented, and he ruptured the membranes and extracted the child by the feet. In case 585, which occurred in 1690, he also ruptured the membranes as soon as they became tense, and again states clearly the reason why he did this. In case No. 624, which happened in 1691, the hemorrhage took place in the eighth month of pregnancy, and here he also ruptured the membranes with the happiest effect. The same practice was adopted with a similar result in case 633.

His fifty-second Aphorism proves that he was likewise fully aware of the importance of rupturing the membranes in the first stage of labour when hemorrhage occurred.

Paul Portal's treatise contains the histories of several cases of uterine hemorrhage depending upon attachment of the placenta to the neck of the uterus. In 1664, he was called to a lady in the eighth month of pregnancy with profuse hemorrhage, and on introducing his hand into the orifice of the uterus, which was open to the size of a crown-piece, he felt a soft body, which was the after-birth, which he gently separated from the body of the uterus, ruptured the membranes, and turned the child.*

In 1671, another case occurred, in which the placenta adhered to the lower part of the uterus, and death took place soon after delivery by turning the child. The following passage, p. 187, proves that Portal knew with certainty that the placenta adhered to the lower part of the uterus. "Je glissai mes doigts dans les orifices où je sentis l'arriere-faix qui se presentoit et qui *bouchoit* l'orifice de la matrice de tous cotez avec adherence en toutes ses parties, excepté par le milieu, qui se trouvoit divisé jusques à la membrane laquelle n'etant pas ouverte, n'y les eaux ecoulées, j'eus beaucoup de facilité à tourner l'enfant."

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* La Pratique des Accouchemens, par Paul Portal. Paris, 1685.

Portal relates another case which happened in 1672, where the placenta also presented, and which he separated very gently from the uterus, because it was glued to the internal orifice. In case 43 the placenta was adherent to the lower part of the uterus, and he also passed up his hand between the uterus and placenta, where the partial detachment had taken place, and brought down the feet of the child. The history of a perfectly similar case is related at p. 233, and to this is subjoined a correct explanation of the cause of the hemorrhage, which occurs in the latter months of pregnancy, from attachment of the placenta to the lower part of the uterus. Five other cases occurred in 1679, and in the last of these he states that he felt the placenta adhering all round to the internal orifice of the uterus. The treatment employed by Portal in these cases did not differ from that which had previously been had recourse to by Guillemeau and Mauriceau. With the effect of rupturing the membranes, in cases of hemorrhage without presentation of the placenta, Portal does not appear to have been acquainted, for he recommends artificial delivery in all the varieties of flooding in the latter months.

Petit examined the body of a woman who had died from flooding when near the full period, and he found the placenta adherent to the neck of the uterus, and exactly closing the orifice except at one part where it was detached, and from thence the discharge of blood had taken place.*

Giffard has recorded about twenty cases of placental presentation, and he was likewise aware of the fact, that the placenta sometimes adheres to the inferior part of the uterus. He concludes the history of Case 115 with the following observation. "I cannot implicitly accede to the opinion of most writers in midwifery, which is, that the placenta always adheres to the *fundus uteri*, for in this, as well as many former instances, I have good reason to believe that it sometimes adheres to or near the *os internum*, and that the opening of it occasions a separation, and consequently a flooding."†

In the history of Case 116, he states, that he told the patient, "that the only way to save her life was by a speedy delivery." When he passed his hand through the *os uteri*, the first thing he met with was the placenta, which he "found closely adhering round the *os internum*" of the uterus, which, amongst many other instances, is a proof that the placenta is not always fixed to the bottom of the uterus, according to the opinion of some writers in midwifery: its adhering to the *os internum* was, in my opinion, the occasion of the flooding; for as the *os internum* was gradually dilated, the placenta at the same time was separated, from whence proceeded the effusion of blood." P. 284.

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* Histoire de l'Academie Royale des Sciences, 1728.

† Cases in Midwifery, by W. Giffard, 1734. p. 278.

The placenta presented in Cases 120, 121, 158, 160, 184, 209, and 224. In the last of these cases, he "felt part of the placenta adhering round about the orifice of the uterus." This case occurred in 1731; and in a further communication, its details, he observes, as he had done before, "I beg leave, before I proceed to give any further account of the delivery, to give my opinion in a point of midwifery, in which I differ from most authors that have wrote upon the subject."

"It is generally believed that the ovum after its impregnation and separation from the ovarium, and its passage through the *tuba Fallopiana*, always adheres, and is fixed after some time to the *fundus uteri*; in this case, the placenta adhered, and was fixed close and round about the *cervix uteri*, as I have found it in many other cases, so that upon a dilatation of the *os uteri*, a separation has always followed, and hence a flooding naturally ensues."

Rœderer has given a clear account of the symptoms and treatment of cases of uterine hemorrhage from attachment of the placenta to the lower part of the uterus. The placenta can be felt, he states, firmly adhering to its inferior segment. He denies the possibility, from any cause, of the placenta being detached from the *fundus uteri*, and falling down to the cervix. In some cases the whole placenta covers the orifice of the uterus; in others, and these are much less dangerous, and may be committed to nature, he states, that it adheres only to the side of the aperture. He has described, with great minuteness, the operation of turning the child, and pointed out its necessity in all those cases in which a large portion of the placenta adheres to the neck of the uterus. He recommends the dilatation of the orifice to be slowly and cautiously effected by the hand, which, he says, is not to be pushed through the centre of the placenta, but through the opening formed by its detachment from the uterus. "Si forte rariori," he observes, "in casu ad pedes usque non amplius iter pateat, forcipe caput solvi potest." The following passage relates to those cases of hemorrhage in which the placenta does not present. "Arte perfectum partum Puzos rejicit, quod (contra experientiam) continuatum ab ejusmodi præcipiti partu hæmorrhagiam metuit; siquidem non pari velocitate uteri vasa clauduntur. Lubentius eo scopo naturalem partum promovet, ut sensim, dum fœtus nascitur, uteri vasa constringantur, hermetice interim ab ipso fœtus corpusculo, ut putat, clausa. Dilatando os uteri partum juvat." He appears to have had little confidence in the practice of rupturing the membranes, for he afterwards recommends trusting to the natural powers of the uterus where the flooding is slight, and where it is profuse speedily emptying the uterus by turning the child.[*]

Levret, in his Dissertation, undertook to prove what had previously been demonstrated;—1*st*, That the placenta sometimes adheres to the circumference of the internal orifice of the uterus;—2*d*, That where this occurs, uterine hemorrhage is inevitable in the latter months of pregnancy;—and, 3*d*, That the only method of obviating this dangerous accident is to deliver immediately by turning the child.[†] He refers to the cases of Van Horne, Schacher, Platner, Brunet, Heister, Portal, Petit, and those which came under his own observation, to prove the truth of his first proposition. He quotes also in its support the opinions of Massa, Drelincourt, Mauriceau, Bidloo, and Noortwyk, that the outer membrane of the ovum adheres by vessels to the whole inner surface of the uterus, and that the spheroidal mass, being equally compressed on all sides by the uterus, cannot be displaced, nor the placenta slide down from the fundus to the cervix, as Deventer supposed. Levret observed that uterine contractions invariably increased the hemorrhage, where the placenta had been implanted over the *cervix uteri*, and that the contrary effect was produced where the placenta had adhered to the fundus, and been detached by an accidental cause. The practice of rupturing the membranes, he considers, therefore, to be as excellent and preferable in the latter case, as it is injurious and prejudicial in the former. In a case which occurred in 1752, he passed the hand through the substance of the placenta, and delivered the child. The child was dead, and the mother died soon after. Levret appears to have been of the first to recommend, as a general rule, forcing the hand through the centre of the placenta, instead of passing it between the uterus and its margin, as Mauriceau and Portal had always done. The occurrence of uterine hemorrhage from placental presentation, he attributed to a developement of the *cervix uteri* from above downward, in consequence of which a separation gradually took place between the two organs, and the vessels of the uterus were exposed.

Although he admits in this Dissertation the superiority of the treatment usually attributed to Puzos, in cases of accidental uterine hemorrhage, yet it is evident he did not adopt this as a general rule of practice. After stating at p. 136, that speedy delivery is necessary in cases of convulsion, he recommends the same treatment in hemorrhage, whether it be from the uterus or from any other organ, if the discharge of blood be profuse.

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[*] Elementa Artis Obstetriciæ, Gotting. 1759.

[†] L'Art des Accouchemens, 2d edit. Paris, 1761. P. 242.

Smellie also knew that the placenta sometimes adheres to the lower part of the uterus, and he has related nine cases to illustrate the fact, and the appropriate treatment. "The edge or middle of the placenta sometimes adheres," he observes, "over the inside of the *os internum*, which frequently begins to open several weeks before the full time; and if this be the case, a flooding begins at the same time, and seldom ceases entirely until the woman is delivered. The discharge may, indeed, be terminated by coagulums that stop up the passage; but when these are removed, it returns with its former violence, and demands the same treatment that is recommended above." Vol. i. p. 173.

Smellie also knew the effect of rupturing the membranes, in cases of hemorrhage where the placenta did not adhere to the *cervix uteri*, and was aware that this practice did not invariably succeed. "If in time of flooding she is seized with labour-pains, or if by every now and then stretching with your fingers the *os internum*, you bring on labour, by which either the membranes or head of the child is pushed down and opens the *os internum*, the membranes ought to be broke, so that some of the waters may be discharged, and the uterus may contract and squeeze down the fœtus. This may be done sooner in those women who have had children formerly, than in such as have not been in labour before. If, notwithstanding this expedient the flooding still continues, and the child is not like to be soon delivered, it must be turned immediately; or, if the head is in the pelvis, delivered with the forceps; but if neither of these two methods will succeed, on account of the narrowness of the pelvis, or the bigness of the head, this last must be opened, and delivered with the crotchet. In all these cases, let the parts be dilated slowly and by intervals, in order to prevent laceration."

As no fact of the slightest importance has since been discovered, relating to the causes and treatment of uterine hemorrhage in the latter months of pregnancy, the history of the subject is here necessarily brought to a close.

14, *Golden Square, London.*

It is due to Dr Robert Renton of this city to observe, that, in a paper published in the forty-eighth volume of this Journal, p. 243, (July 1837,) he first directed the attention of the profession to the original observations of Paul Portal on implantation of the placenta, over or near the *os uteri*, and its effect in producing unavoidable hemorrhage, with the practice to be followed, as recommended by that practitioner, by Giffard, Roederer, Levret, and Smellie. That paper, with the present by Dr Lee, who traces the knowledge of the facts to Guillemeau in 1609, and Mauriceau in 1668, may be regarded as completing the literary history of this important anomaly, and the modification of practice which it demands.—EDITORS.

Art. VIII.—*Cases of Morbid Adhesion of the Placenta to the Uterine Surface, with Remarks.* By John Renton, Surgeon, Penicuick.

Having had frequent opportunities of separating adherent *placentæ* from the uterine surface, I have selected the following cases from my note-book, in the hope, as their histories are very fully narrated, in reference to the causes, symptoms, and mode of extraction, that a communication on so important a subject might not be unacceptable to the readers of your Journal. The three first afford very good examples of *metritis*, occasioned by external injury, spreading into the substance of the *placenta*, and exhibiting the different products to which the inflammatory action gave rise. The fourth appears to be a well-defined instance of the constitutional tendency in the *uterus* to assume organic disease, without any apparent or external cause. I have applied the word constitutional, to mark the difference between the nature of the organization connecting the uterine and placental surfaces in this case, and what I have described in the three first cases, as originating in and dependent upon disease of a more local character. In all these cases, the *placenta* was wholly adherent, or very nearly so. In the fifth case, the morbid adhesion was confined to a small portion of one of its lobes, and the seat of the partial inflammatory action cannot be satisfactorily traced. There was nothing peculiar in the case during the whole period of pregnancy, so far as I could learn, to induce the apprehension of organized placental adhesion; and as the individual never received any external injury, or complained of any pain in the uterine region, it is impossible to decide, especially from the want of necroscopic inspection, whether the effusion of coagulable lymph proceeded from placentitis as an idiopathic disease, or was the result of inflammatory action originating in the uterus, and propagated to the contiguous surface of the *placenta*. This partial and intimate adhesion between the uterine surface of the secundines and the inner corresponding surface of the uterus, is a morbid condition of these parts, which, on account of its practical bearings, should never be lost sight of, as it forms one of the most formidable and fatal causes of uterine hemorrhage, after the expulsion of the child. The importance of these cases will justify me in going into some details in reference to their management; and as they illustrate the mode of extraction which I have long and successfully practised, a particular description of the manual operative means becomes the more necessary.

Case I. *Complete adhesion of the placenta to the fundus uteri, in which the funis was remarkably affected by the uterine contractions, long after the birth of the child.*—This case occurred in August 1817. The subject of it was a very healthy, muscular, young woman. The first and second stages of labour (which was a first one) were tedious and unusually severe, from undilatability of the *os uteri*, and rigidity of the external parts. The completion of the third stage not being accomplished within the hour, I proposed acting according to the useful general practical precept inculcated by my preceptor, Dr Hamilton, Edinburgh, of effecting the removal of the placenta by manual assistance. I had previously tried every means usually recommended, such as making the patient cough, sneeze, and bear down during the after pains, which were frequent and severe, while I gently pulled by the funis. But finding that the *os uteri* had greatly contracted, while the uterine tumour was round and hard, and much larger than usual, I concluded that the placenta would not easily be removed, and as the plea of inexperience was successfully urged by the relations against my further interference, I cordially concurred in the propriety of their proposal to send for my father.

More than another hour elapsed before the messenger arrived with a note from him stating, that he believed his assistance was not required for the purpose of extracting the secundines, as he had often been needlessly sent for, to remove what were erroneously supposed to be their morbid adhesion, while their exclusion was merely prevented either by their impaction in the upper part of the vagina, or by a portion of them being retained by contraction of the *os uteri;* and he concluded by advising me how to proceed under these circumstances. I immediately despatched another messenger, with a letter, urging his prompt attendance, and briefly stating my conviction, that the symptoms unequivocally denoted adhesion. I mentioned particularly that there had been no lengthening of the funis; that its insertion into the placenta could not be felt; and that retraction followed on the cessation of traction.

He arrived about four hours after the expulsion of the child, and found the uterine tumour as I have described, and that there had not been the slightest hemorrhage. A large opiate was administered, and until its narcotic effects were produced, I went over the history of the case, and took occasion to advert to a symptom which had never come under my notice before, as I then considered it might be diagnostic of placental adhesion, viz. that, in attempting to remove the placenta, the cord, so soon as I tightened it, became firm and distended, and when I slackened my hold, it became as speedily soft and relaxed. This alternate state of distension and flaccidity continued for more than an hour, and was reproduced equally by continued traction, as by the recurrence of uterine contractions. The introduction of the hand into the *vagina* occasioned such violent pain, that my father was forced to desist from attempting it, until twenty ounces of blood were rapidly taken from the arm, and a state of syncope induced; but, notwithstanding, he experienced the greatest difficulty in completing the operation, not less from the swelling of the vaginal tunics and the contracted state of the *os* and body of the *uterus*, than from the intimate adhesion of the secundines to the uterine surface. These he found wholly adherent at the *fundus uteri*; and they were, as I have had occasion to remark in similar instances, unusually large, but unaltered in structure. The operation was not accomplished in less than an hour.

The pain which the woman suffered in the dilatation of the contracted *uterus*, was greater, she said, than what she underwent in the two first stages of labour, and was far worse to bear. Her sufferings made on my mind a strong impression of the evil consequences arising from protracted assistance, and for these I felt the more, that I considered myself to blame as being in part their cause, and that I stood in the anxious position of a spectator disengaged from an operation and only watching its incidents. The plan recommended and practised by Dr Denman, was followed by my father, of searching for the edge of the placenta, and effecting the separation between it and the uterine surface by the blunt end of the fingers.[*]

Notwithstanding the long-continued manipulations which were found necessary to the complete removal of the placenta from the uterine surface, no attack of *metritis*, nor even of uterine irritation followed the operation. The lochial discharge was perfectly natural, and a good recovery ensued.

The cause of the morbid adhesion the patient imputed to a severe injury on the lower part of the abdomen, which she received from a fall about the period of quickening. She never was wholly free from pain in that region during the subsequent period of pregnancy, and had a presentiment that "the after-burden was growing to her side."

More than twenty years have elapsed since the occurrence of this case, and although both the woman and her husband have enjoyed uninterrupted good health, it is deserving of notice that she has never again conceived. The conjecture is by no means improbable, that obliteration of the Fallopian tubes might be an effect of the same inflammatory action, induced in the *fundus uteri* by the external injury the woman received, which had occasioned, by the effusion of coagulable lymph, the morbid connection between its internal surface and the placenta. It is not easy to account for the subsequent sterility in any other way, as the *uterus* appeared otherwise free from organic change by the absence of local pain, and by the regular performance of its monthly functions.

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[*] Mr White of Manchester, in stating the disadvantages attendant on deferred manual extraction after delivery, observes, "by waiting an hour or two, you lose the opportunity of extracting the secundines, the womb contracting either at its mouth, or across the middle, like an hour-glass, by which contraction, laceration is endangered, if the hand be forced into the uterus." (Treatise on the Management of Pregnant Women, p. 84.) Perfect has collected evidence on the same subject from the writings of Mauriceau, Burton, La Motte, and Exton; and has recorded a most extraordinary case, in which the placenta was retained for several days in six successive labours in the same individual, from contraction of the *os uteri*. (Cases in Midwifery, Vol. i. p. 91.) Giffard has practically illustrated this in Cases 31, 72, 74, 92, 127, 134, &c. in which the pain occasioned to the patient, and the difficulty experienced by the operator, by the introduction of the hand through the *os uteri*, a few hours after delivery, strongly corroborate the necessity and utility of early interference for the removal of the placenta. (Giffard's Cases in Midwifery, by Hody, 1734.) For a "very singular contraction of the *os uteri*, in half an hour after the birth."—See Dr Johnson's Treatise, p. 206.

The long-continued, alternately distended and relaxed state of the funis, is too uncommon an occurrence to be passed over without remark, for the circumstance has never again come under my own observation, nor, so far as my reading extends, has it been noticed by any obstetrical author. Dr Dewees records a case of the pulsation in the *funis umbilicalis* continuing for six hours and a-half after the birth of the child, " during which time the patient had no flooding nor pain," and " the cord was suffered to remain entire." I do not mean to institute any analogy between the two cases; for not the slightest exists.

The circumstance which Dr Dewees relates is undoubtedly very extraordinary, of the fœtal circulation continuing after the extra-uterine circulation had been in part established by respiration. The imperfect manner in which both processes—the maintenance of the circulation between the mother and child, and the respiration of the latter—were carried on so long, can only be explained by supposing that organic disease, or rather malconformation, had existed to a very unsual extent. I have been induced to allude to this case, merely on account of the following observations. " I conceive," says Dr Dewees, " the gradual diminution that was observed in the force of the umbilical arteries, marks the uterus recovering its tonic contraction, for so soon as it began to pucker up, the circulating force both in it and the placenta was diminished; at length it contracted so much as to entirely impede that through the placenta, and eventually throw it off."

In the same way, it may be supposed in this case, that the violent uterine contractions, compressing as it were the uterine surface of the adherent placenta, might dislodge the blood from the placental cells, and force it into the umbilical vein; for as long as the uterus continued to diminish its volume by contraction, so long this peculiar condition of the funis returned; and traction produced the same effect, so long as it excited uterine action. The action of the uterus may therefore, if my explanation be admitted, be not inaptly compared to the systole of the heart. But in whatever way the peculiar state of the *funis* which I have described may be accounted for, the fact itself, though singular, is undoubted; for I could not possibly be mistaken regarding it.

Case II. *The placenta wholly adherent to the posterior wall of the uterus.*—Mrs H. was delivered of her first child, a boy, May 21, 1838. The labour was rather severe. After the completion of the second stage, I attempted unsuccessfully, by external compression with the hand, which I continued to apply for nearly an hour, to effect a good contraction. Failing to accomplish this, notwithstanding I was assisted by regular although slight action of the uterus, I was led to apprehend, especially in the total absence of discharge, that the cause of the undiminished volume of that organ might proceed from complete adhesion of the placenta. This occurrence was rendered the more probable, for she stated, that, about the middle of utero-gestation, she had while in the cow-house been squeezed between two oxen, and after falling had received several severe contusions upon the lower and fore-part of the belly, in consequence of being trodden upon. Great local pain supervened, and she was confined to bed for nearly fourteen days, but underwent no treatment. For a long period she was unable to lie on the left side.

I was confirmed in the opinion that morbid adhesion had taken place, when I introduced my finger along the funis to the upper part of the vagina, and found that no portion of the secundines had escaped from the uterine cavity. Although the cord was merely used as a director, and not as a tractor, and I only moved it in the slowest and gentlest manner from side to side, in order to ascertain if the placenta was detached, yet its placental attachment had been so slender, that its disruption could not possibly be prevented. I was satisfied that the extracting force I used was not sufficient, under ordinary circumstances, to have caused such an accident.

For the safe and speedy removal of the placenta, no alternative was now left but to introduce the hand. The first difficulty I experienced was in dilating the *os uteri*, which was very much contracted. The body of the uterus possessed, as Dewees would express it, great tonic contractile power, which very much retarded my manual operations. The placental mass was unusually long, much thickened in substance, and indurated. It extended from a little above the cervix, posteriorly along the parietes of the uterus, nearly to its fundus. To this part of the uterus I passed my hand before commencing the process of detachment, for the purpose of removing any spasmodic contraction, which not uncommonly is found in such cases, and of ascertaining the locality, if any existed, where the secundines might be partially separated from, or be less firmly adherent to, the uterine parietes. I carefully examined the whole placental surface, but could not find any separated portion. The slow expulsion of the child, and the constant application of external compression afterwards over the uterine region, had prevented the occurrence of irregular or spasmodic contraction. The unbroken and universally firm attachment of the placenta rendered the place and the primary stage of detachment objects of some difficulty.

I commenced the separation at the placental margin, beginning from above, and proceeded gradually downwards. The process was tedious, in consequence of the intimate union of the placental and uterine surfaces throughout their whole extent. I used the broad side of the fore-finger to detach the adherent surfaces, carrying it between them in the slowest and most gradual manner, and employing what more disjunctive power I found necessary for the removal of the less easily separable portions, by fixing between my thumb and the lower part of my fore-finger the last detached portions of placenta. By this means I was both enabled to use more equally applied force with the first phalanx of the finger, in disuniting without lacerating the adherent surfaces, and to know the direction in which that could be done without injury to the uterus. Although the uterus possessed great contractile power, which by compression deprived me at intervals of the free use of my hand and arm, yet the operation did not last beyond forty minutes, and was performed with much less suffering to the patient than in the subsequent case, and with comparative facility to myself, in consequence of the more favourable position of the placenta for removal by the right hand.

A drachm of laudanum, with a small table-spoonful of brandy, was administered before, and repeated after the operation. I invariably employ this prescription after every labour remarkable either for its severity or duration, proportioning the dose to the urgency of the symptoms, and have found it to be a useful and necessary adjunct to the ordinary treatment in all cases of uterine hæmorrhage, when the action of the heart and arteries requires to be moderated, whether the frequency of circulation depends upon nervous excitement, produced by mental anxiety or bodily suffering. The patient's mind in this case was greatly affected with apprehension and fear, in consequence of the prevalence of malignant puerperal fever, of which disease a near relation of hers died about the period of her confinement. Dreading the occurrence, under these circumstances, of internal hemorrhage from secondary dilatation or enlargement of the womb, I did not leave the patient for more than two hours after delivery, after which time I considered all danger from such a cause was removed.

Considerable uterine irritation supervened on the second day; but under my colleague, Dr Madden's care, the inflammatory symptoms, which were not of an acute character, were soon subdued by leeches and fomentations, and a perfect recovery followed. This is the only case in which I operated for the removal of an adherent placenta, where any tendency to hysteritis occurred. I do not, however, impute the slight attack of that disease to the manipulations used, for a very great proportion of the women whom Dr Madden and I delivered about that period were affected with similar complaints of the uterus. That organ seemed to be the seat of a puerperal epidemic, originating in general, not in local causes. "The influence which the atmosphere exerts over the nervous and sanguiferous systems, more particularly after delivery, and especially where there exists a predisposition to morbid action, has long been matter of observation."* The lochia were suppressed for a day, but they returned and continued for the usual period, unmixed with any purulent or offensive discharge.

I have been particular in describing the operation, because high authority has suggested another mode of extraction, which, in my opinion, is objectionable for the best reasons, that it is both impracticable and unsafe. I refer to the proposal of making pressure upon the substance of the placenta, bringing its circumference towards its centre, and detaching leisurely and carefully all that can be separated by this manipulation. In a contracted state of the uterus, such as has oftner than once occurred to me, the second part of the process recommended could not be effected by the hand; and though it were possible it could be done, inversion of the uterus might either follow, or portions of adherent placenta would be left behind, and occasion afterwards fatal hemorrhage, as occurred in Case 5th. I need scarcely add that, in such an uncontracted state of the uterus, as would admit the hand to move so freely within the cavity, as to be able to draw up the circumference of the placenta towards its centre, the exposing at once the open large mouths of the uterine sinuses, would not only be a rash act, but an unadvised experiment against every acknowledged obstetric precept. The gradual and slow mode of detachment I practised was followed by corresponding contractions of the uterus; and when the process was completed, the uterine cavity was nearly obliterated. I found little difficulty in withdrawing my hand and the detached placenta, which was below its hollow, through the expanded *os uteri*.

Ramsbotham says, there is the greatest difficulty in insinuating the fingers between the placenta and uterus, and even in distinguishing by the finger what portion is uterus and what placenta, especially in a contracted uterus, where the hand has little room for action.—Pract. Observ. Part i. p. 75. In the 175th case recorded by Giffard, of a "placenta left in the uterus, and the cord broke off," the uterus was very much contracted, so that he could not without difficulty pass in his hand; the placenta was wholly environed by the uterus, so that he could not readily move his fingers to separate it.

CASE III. *The placenta partially adherent to the uterine surface by false membrane.*—This organized connection by means of false membrane, is a morbid condition of parts, which is very rarely to be met with. Simple adhesion between these bodies caused by the effusion or secretion of coagulable lymph from inflammatory action is found to exist in every variety of strength, extent, and intimacy. In this case the inflammatory effusion of lymph had been changed into a kind of morbid cellular structure in some parts of the adherent surfaces, so that the placenta seemed to be united to the contiguous uterine parietes by means of filamentous bands.

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* Vide Ingleby on Uterine Hemorrhage, 1832, page 4.

Mrs M., about 30 years of age, a healthy person, but of a nervous temperament, was confined on the 6th of November 1837 by the birth of her seventh child after a very easy labour. With the exception of the first, each labour had been accomplished in so short a period that she never solicited medical assistance. In the early months of her last pregnancy, she received, when in bed, a severe kick on the pubic region, from one of her children, which occasioned great local pain. Within twenty-four hours uterine action supervened, and considerable hemorrhage *per vaginam* took place on the following day.* I learned from Mr Monteath, who had the management of the case, that several ounces of blood were lost. She was bled at the arm by him, and underwent very active treatment, which was found necessary for allaying the inflammatory symptoms which arose, and preventing the miscarriage with which she was threatened. She was long confined to bed, and was never free from a burning hot pain in the uterine region during the whole course of pregnancy. From this long-continued and constant pain, she had so strong a presentiment that something wrong would occur, that she sent for Mr Monteath as soon as she was taken in labour. In his absence I attended the case. I learned upon my arrival, that the child had been born about three hours, that there had been a great deal of abdominal uneasiness, with regular recurrence of after-pains, and that the placenta had not come away. The patient was labouring under great nervous irritability, but the pulse was good, and there was no external flooding.

Externally the uterus felt very irregularly contracted, bulky, and flaccid, extending from the pubis to the *scrobiculus cordis*. External compression neither altered its shape nor diminished its volume; the only effect which it produced was the expulsion of some large coagula.

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* It appears probable that detached portions of placenta never regain their connection with the uterine surface. If natural agglutination takes place, it must be, as Mr Ingleby observes, at a very early period. Mauriceau, Leake, and Dewees think that the separated portion of secundines never afterwards adheres; but when the separation depends on uterine action induced by external injury, there is reason to believe that morbid cohesion between the disunited surfaces may be the product of an attack of subacute or chronic metritis occasioned by the accident. Mr Ingleby does not mention the cause of the flooding in the case he refers to, in which severe flooding began in the third month, and continued recurring every second or third day, until the seventh month, when the lady was delivered of a very feeble infant, which only lived an hour. "The great mass of the placenta seemed to have undergone premature separation." The placenta presented no such appearance in this case; and the subsidence of the hemorrhage might in a great measure be owing to the rapid succession of congestion and inflammation in the uterine parietes.

This condition of the uterine tumour seemed to me to have been occasioned partly by the rapid and unassisted expulsion of the child, and the want afterwards of equally applied pressure by the hand over the abdominal parietes.

Upon internal examination, the vagina was found almost filled with small coagula, the *os tincæ* high in the pelvis, much contracted, and the edge of the placenta grasped by it, presenting like the *liquor amnii* through the membranes in the beginning of labour. The funis had been disrupted by a female attendant, (who had delivered her on former occasions,) in her attempts to remove the placenta. The introduction of my hand into the vagina caused the expulsion of the coagula within it, which was followed by a great discharge of fluid blood from the uterine cavity. Five minutes elapsed before I effected such a dilatation of the *os uteri* as allowed the passage of my hand through it. The chief obstacles were the unusually great depth of the pelvis, and the high situation of the *os uteri* within it, which was almost wholly closed, and very undilatable. The rolling of the uterus* has often been noticed; and I am convinced from its recession before the hand in the present instance, that its connection with the vagina, (which seemed greatly on the stretch,) would have been endangered by dilating the parts rashly, especially if the direction of the manual efforts had been made propulsively upwards, and not in the most gradual and expansive manner. The interior of the uterus had the shape of a crescent, the large segment of the circle being formed by its anterior parietes, and afforded a very good example of the posterior longitudinal contraction. I soon discovered that the deliverance of the placenta did not depend on the natural contraction of the *cervix uteri.* About one-fourth of its lower portion was detached, and the remaining part adhered not closely and intimately, but by means of detached bands, from below the middle along the anterior wall of the uterus, which was puckered transversely and very irregularly, forming a striking contrast to the posterior side, which was uniformly smooth, and from contraction firm and greatly thickened. The *fundus uteri* illustrated the species of cyst or cell described by Peu. From its situation, which was posteriorly downwards, as it formed the descending part of the uterine curve, I had considerable difficulty in reaching it, and in overcoming the spasmodic contraction which had formed it. Within this encystment no part of the placenta was lodged. The bands by which it was united to the uterine surface, felt like dense cellular membrane, and of the consistency of those adhesions, by which the *pleura pulmonalis* is connected to the *pleura costalis* after inflammatory attacks. These were so numerous and firm, and equally strongly adherent to the uterus and placenta, as to require more than an hour for their detachment, which was effected by cautiously pinching each of them separately off, between the fore-finger and thumb, close to the uterine surface. Insinuating first, and gliding afterwards the hand between the membranous adherent surfaces, was a mode, which, although usually recommended, would have been totally inadequate for their separation.

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* See Smellie's 3d, 4th, and 6th Cases, page 392, Vol. ii.

In this case the first step of extraction was reversed, in consequence of the placenta being partly separated at its inferior margin, where I commenced detaching upwards. Though the operation, Baudelocque justly observes, is easier when the secundines are in part detached, than when they are wholly adherent, it is also a more pressing circumstance, on account of the hemorrhage which almost always accompanies it, whereas in the latter case that accident does not exist.

When the placenta is adherent to the posterior uterine wall, it is generally directed, that the right hand be employed for its extraction; and the left, when the attachment is anterior. This will be found, however, to answer only when either the adhesion is complete, and you have it in your option to commence detaching from above, or when it is partial, and a natural separation has taken place on the superior edge of the placenta. But when its inferior side is separated, and the upper portion adherent, the operation will be greatly facilitated by reversing the hands.

I experienced the greatest disadvantages by operating under these circumstances with the left hand, which I introduced under the belief, from external examination of the uterine tumour, and in the absence of all discharge, that the placental adhesion was anterior and complete. As the existence and site of the partial separation could only be ascertained by internal examination, I was prevented from rectifying my unavoidable mistake, by withdrawing the left and introducing the right hand, in consequence of the great irritability of the patient, and her extreme unwillingness to submit to the operation. I was personally unknown to her, and she had most unfortunately mistaken me for another practitioner, against whom (although she never saw him) she had imbibed the most inveterate prejudice. It is impossible for me to describe the extent of resistance I met with on this account. I nevertheless persevered, amidst her cries and abjurations, and in opposition to interruptions more discouraging than I ever encountered before, and which greatly augmented the inconveniences I laboured under, of operating with the left hand.

In enumerating the difficulties inseparable from an operation, the case would be incomplete, without mentioning these incidental obstructing causes. By their omission, the opportunity would be lost of enforcing the value of the obstetrical maxim "nec temere nec timide;" for on its practical application, the safe and successful result of manual interference chiefly depends, and by it alone, we are enabled to overcome every accompanying embarrassing symptom.

Mr Monteath attended the patient afterwards, and from him I learned, that she made a good though rather a slow recovery. During the puerperal state the lochia continued natural, and there were no symptoms of any uterine affection.

The danger of occasioning uterine irritation and structural lesion has been urged against the mode of extraction followed in this case. Experience has led me to believe, that such bad effects are more apprehended than met with.[*] Where the proper precautions necessary on these emergencies are observed, they can be but of very rare occurrence, a result in a great measure attributable to the peculiar organization and functions of the uterus itself. If the organized connection between the uterine and placental surfaces has not degenerated into cartilaginous or osseous structure,[†] (and to such organic changes my observations are not intended to apply, for these happily have never come within the range of my practice,) I consider that every portion of adherent secundine can and ought to be removed by manipulation, and that their separation ought on no pretext to be left to be effected by the slow and hazardous processes of absorption and suppuration, processes which not unfrequently terminate in fatal uterine hemorrhage, and almost always are accompanied by a greater or less degree of constitutional irritation and destructive local disorganization.[‡]

The want of external discharge in this case, and its total absence in the other two cases, do not correspond with Dr Hamilton's experience in morbid placental adhesions. The following remarks, it is obvious, can only refer to cases of partial and not of complete attachment. "When the placenta morbidly adheres," he says, "a gush of blood follows the birth of the infant, uterine contractions succeed, each pain being usually followed by another gush, while the uterus feels contracted into a round form, and yet there is no lengthening of the cord. In the whole course of the author's practice, he has only met with one case where these symptoms did not occur, and that case happened about twenty years ago." (Practical Observations, Part I. page 284.)

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[*] Giffard's authority may be cited in confirmation of this, no bad consequences having followed manual extraction, which was had recourse to in cases 74, 75, 79, 134, 135, 178, &c. where the placenta was closely adherent. This practice was "to pass the end of his fingers between the uterus and the placenta, beginning their separation at that place, where the latter adhered most loosely." More recently Dr Ramsbotham has operated in the same way, and with similar success. See cases of adhesion in Part I.

[†] In the cases of ossified placenta stated by Gooch, and referred to by Merriman, the fact in a practical point of view is most important, "that there did not appear to have been any difficulty in the separation." The instances which have come within Dr Merriman's knowledge only indicate "that the placenta has perhaps adhered rather more firmly." Merriman on difficult Parturition, p. 156. In such cases, it appears probable, that the calcareous, cretaceous, or ossific matter is imbedded only in the substance of the placenta, and has not extended to the uterine surface.

[‡] In Perfect's 136th case, the patient was delirious and died of a putrid fever on the twentieth day from retention of the placenta'; and the 11th case recorded by Mr White, Manchester, terminated in the same way from a similar cause.

There is reason for assuming, when the uterus has contracted into the round form described by Dr Hamilton, 1*st*, That the adhesion is only very partial; and 2*dly*, That the *fundus uteri* is the place of attachment. But, on the other hand, when adhesion is complete or very general, and especially when it is wholly lateral, and the effect of external injury, it is easy to understand, why the uterine tumour should present a totally different shape. When the accident occurs in the early months of pregnancy, and chronic metritis succeeds, adhesion and subsequent enlargement of the placental mass are very probable consequences of the inflammatory action, and as these prevent the uterus from regularly decreasing its volume, the bulky and irregularly contracted condition in which I found it in the two last cases is readily explained. A peculiar modification of internal flooding might have taken place in the last case, if the placenta had been adherent around the whole circumference of its edge, for, as the morbid adhesion was not intimate over the whole extent of the uterine and placental surfaces, but only existed in numerous patches, the separation of the placenta having been effected in the intermediate spaces by irregular uterine action, it is quite clear, that a fatal effusion of blood might have been contained and concealed within this artificial cyst.

Dr Hamilton mentions, that, "in only one case, he found it (the morbid change) extended to the whole circumference of the placenta to the breadth of an inch. The middle part was of the natural texture. It was in that case that no hemorrhage followed the birth of the infant, and that the cause of retention was not understood till the hand was introduced into the uterus."—(Pract. Observat. Part I. p. 286.)

Dr Blundell acknowledges, that "he at one time imagined the offensive uterine discharge was caused from violent efforts having been made to extract the placenta; but he has since found it to occur where no force was used."—(Lectures, p. 156.)

Case IV. *Adhesion of Placenta with structural change.*—The two preceding cases illustrate the different states in which the uterine and placental surfaces were found preternaturally united, after an attack of subacute metritis induced in the early months of pregnancy, by external injury. The placenta was not only much enlarged in volume, but firmer in consistency. Its reddish appearance throughout the whole parenchyma, and in the membranes investing both the fœtal and uterine surfaces, afforded conclusive evidence of the change having been produced by increased vascular action. This action, however, appeared to have ceased with its only products,—the effusion of lymph, and the formation of false membrane,—processes by means of which the adhesion of the contiguous surfaces was effected. The separation of these by manipulation had no effect in exciting renewals or recurrences of the inflammatory action. This consequence might, to a certain extent, have been prevented by the antiphlogistic offices of the lochial discharge; but the total want of any tendency in the uterus to resume the morbid action is more satisfactorily explained, in my mind, on the ground, that the nature of the affection was simple and uncomplicated, and had originated in a local and accidental cause.

The new organization of the connecting medium between the mother and fœtus did not affect, in the slightest degree, the growth and developement of the latter, as both the children, when born, were strong and healthy. In the three cases of ossified placenta which occurred to Dr Monro, "the children were thin, but in every respect healthy." Where enlargement of the placenta does not proceed from an inflammatory process, and in those instances I have found it generally to occur in women who have formerly, although at a long period back, laboured under symptoms of secondary syphilis, there is not the same vascular congestion of the parts; but the placental mass seems to have undergone a species of white ramollissement, having the appearance throughout of a soft, fatty, adipose tumour. The structural change almost always interrupts the progress of utero-gestation, the death of the fœtus occurring between the fifth and seventh month, and abortion following that event in a few days.

In this fourth case, the adhesion was complete, but the morbid action, which occasioned it, seems to have been confined to the lining membrane of the uterus. The patient had not received any external injury; and during the whole course of pregnancy, although there was occasional abdominal uneasiness, the constant pain experienced in the uterine region in the three preceding cases was wanting. There was not the slightest ground for suspecting venereal taint. In this case the morbid action, on which the adhesion depended, originating in general and constitutional causes, did not cease with the separation of the united surfaces, as indicated by the supervention of puerperal irritative fever, and the admixture of fetid offensive matter with the lochial discharge.

In April 1822, I received from a neighbour practitioner a message requesting my immediate assistance in a case, where a Mrs T., soon after the birth of the child, had fallen into an alarming state of syncope from uterine hemorrhage. On my arrival, my friend informed me that she had been delivered about an hour and a half before of a healthy infant (a female,) after an easy and natural labour. The funis had been twice coiled round the child's neck, and was very short,—circumstances to which he attributed its separation from the placenta, that unpleasant accident having occurred without almost any traction having been used. As the placenta had not descended within the reach of his finger, he introduced his hand into the uterus, to the upper and anterior parietes of which he found the placenta adherent. He endeavoured to excite uterine action by pressing on the surface of the placenta, and effecting its removal by this means. But its substance was so thin and lacerable, that it gave way upon being gently grasped, and the fingers perforated it without any separation following. He succeeded, however, in detaching a considerable part by pulling it slowly from the uterine surface, but desisted in his attempts to remove the remaining portion, partly in consequence of the patient becoming suddenly sick and faint, and partly from the difficulty he felt in distinguishing between its soft and spongy texture, and the coagula with which it was surrounded.

The patient exhibited all the well-known symptoms arising from a sudden and profuse loss of blood, viz. pale countenance, small feeble pulse, vertigo, hurried breathing, &c. I examined the portion of placenta brought away, and concluded that nearly one-third still remained *in utero*. I administered a large dose of laudanum and brandy, which was with difficulty swallowed, the patient being so oppressed with sickness, as to be able to articulate only in whispers. Without moving her from, or stirring her on the spot where she lay, I passed my right hand through the vagina into the uterus without the least hinderance, as these two organs seemed to form but one continuous canal. This might have been partly occasioned by the uncontracted state of the parts from hemorrhage; but the same circumstance has been noticed by Madame Boivin and M. Dugès after repeated labours (and in this case it was a tenth labour,) where the projection of the *cervix uteri* has entirely disappeared, and the orifice has formed the fundus of the vagina, in the form of a funnel. I allowed the right hand to remain, without having recourse to manipulation, nearly half an hour *in utero*, and used the other hand in compressing the uterine tumour externally. By that time the uterus began to act, and the patient to revive from her depressed state.

The detachment was then slowly effected in the usual way. I was occupied nearly twenty minutes in completely clearing the uterine surface, in consequence of the whole substance of the placenta, as it separated from it, breaking down into small pieces under my finger.

The small coagula embarrassed me a little, and considerably prolonged the operation. But I made no hurried attempts to shorten its duration; for, confiding in the hand, as the best uterine irritant, I considered, that on its presence within the uterus, the safety of the poor woman, under her present circumstances, in a great measure depended. I therefore only removed it for the purpose of emptying the uterus of its contents, and immediately introduced it. I kept it *in utero* for some time after a satisfactory contraction had taken place, as I dreaded, in the exhausted state of the patient, the danger of secondary enlargement of the uterine volume from relaxation—an occurrence which cannot be too much guarded against, nor too much condemned, as it almost invariably proceeds from negligence on the part of the practitioner. * Too much attention, I conceive, cannot be paid to this source of danger, for, so far as my experience goes, the great proportion of deaths after delivery, from uterine hemorrhage, arises from this particular cause. No such event supervened in this case, nor, indeed, any untoward symptom until the second day, when the uterus became sensible to the touch, considerably tumefied but soft, with suppression of the lochia, and pains shooting towards the groins. There were occasional grinding pains in the abdomen indicating irregular uterine action, without the expulsion of any coagula or remains of the placenta. By means of hot fomentations, the threatened symptoms of uterine phlebitis went off on the fourth day, when the lochia returned exceedingly fetid, and of a brownish colour. It is unnecessary to trace the symptoms as they were developed, and the treatment pursued; suffice it to say, that the progress of recovery was protracted to nearly a year. For two or three months, the discharge from the uterus irritated greatly the vaginal tissues, was sero-sanguineous, purulent, and sero-purulent, varying in colour from dark-brown at first, through all the shades of yellow to pale green. After this, the local symptoms abated, but the constitutional symptoms increased; the shivering fits became more severe and regular, accompanied with intense heats, acute headachs, dryness of the skin, colliquative purgings, and general emaciation of the body. Her countenance expressed great anxiety, and had a chlorotic appearance. To these supervened dyspnœa, with hard dry, hollow cough, and constant pain in the right side of the chest, while there was dulness on percussion, and the patient appeared to be fast sinking, when she expectorated, in a violent paroxysm of coughing, about half a pint of fetid sanguineo-purulent matter. After this the severity of the constitutional symptoms abated, and the offensive discharge from the lungs gradually diminished in quantity. Tonics and the free use of wine were prescribed; and by the end of six months from the commencement of the evacuation of the vomica, her health and strength were so far restored, that she was able to superintend her domestic affairs.

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* "The womb may be contracted and dilated, and these states recur alternately for an hour or two after delivery; here, of course, there is danger of hemorrhage. I have lately seen a case of this kind with Mr Wilson of Northampton Square, which required pressure with the hand for three hours, ergot and cold affusion."—Ryan's Manual, page 490.

This woman unfortunately became pregnant again, and was delivered at the full period about two years after her last confinement, of a healthy infant, (a female). At her urgent request I attended her in labour, and found to my surprise that I was under the necessity of having recourse to the same manual means as on the former occasion, for the removal of the placenta, which was wholly adherent, and had undergone a similar softening degeneration of its substance.[*] No local symptoms of uterine irritation, congestion, inflammation, or granular ulceration succeeded the operation. The right lung was the seat of the new, or, to speak more correctly, of the renewed destructive disorganization. Although no decided symptoms of phthisis were manifested during utero-gestation, she died in six weeks after giving birth to her child, under all the aggravated symptoms, viz. constant cough, copious purulent expectoration, hectic fever, and which characterize the worst forms of that disease.

As dissection of the body was not permitted, it is impossible to say what structural change or if any existed in the uterus. In the absence of all positive evidence, I am disposed to regard the adhesion between the uterine and placental surfaces, as originating in chronic inflammation of the interior surface of the womb,—the effect of puerperal congestion of the parts,—modified by a strumous habit. Experience tells us, that in such habits, there is a great tendency to assume organic forms of disease, and the existence of such a tendency is more than to be inferred from the symptomatology of the present case.

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[*] In proof of the constitutional tendency in some organs, from particular exciting causes, to assume the same structural form of disease, Dr Gooch mentions, that he has met with ossified placenta occurring three times in the same patient. I might cite in further proof of this, the circumstance my father told me, of his having occasion, in the case of a Mrs L., to assist manually in the extraction of the placenta, which was partially adherent, and accompanied with alarming hemorrhage, in her five last (consecutive) labours. The same occurrence has been noticed by practical writers. A similar case is recorded by Dr Hamilton. He says, "some individuals seem to have a peculiar tendency to this unfortunate change in the secundines. Thus it consists with the author's knowledge, that several mothers of a large family (viz. women who have had ten or twelve children,) had in the course of their child-bearing life, incurred three or four or five times, great danger from this circumstance." —Hamilton's Practical Observations, part i. p. 286–7.

It does not appear, however, that the manipulations used for the removal of the placenta either increased or accelerated that tendency to diseased action. On the contrary, I am persuaded, that in such a constitution, the presence and adhesion of a portion of the placenta, and the accumulation of putrid coagula in the uterine cavity, would have acted by direct irritation, in inducing a formidable attack of metritis or uterine phlebitis.

Regarding the softened state of the adherent placenta, various doctrines have been taught by different practitioners.

"Sometimes the placenta adheres," Dr Burns observes, "when it is unusually soft and tender, and then we must, with peculiar care avoid hasty efforts, by which the placenta should be lacerated, and part left behind, which should be hurtful afterwards; whereas by a little more patience, and gentle pressure on the surface of the placenta, the uterus might have been excited to throw the whole off."—(Principles of Midwifery, p. 373.)

Gooch, who followed the same practice, states (Compendium, p. 177,) "when the placenta is clawed from the surface of the uterus, there is generally a piece, or shreds of it left behind, which will undergo putrefaction, occasion a highly offensive discharge, perhaps irritative fever, and even death." These accidents are, however, much more likely to happen, in my opinion, from pressure made on the surface of the placenta, to cause its separation, by Gooch's claws, when that body is unusually soft, than by insinuating the fingers between it and the uterine surface. The evils he fears from clawing, must, indeed, necessarily result, if clawing be not had recourse to. The great objection to Dr Burns's plan is, that it is wholly inefficient. Dr Ramsbotham, who inspected the bodies of three women who died from adhesion of the placenta, for the removal of which no effective manual attempts have been made, observes, "that each case convinced him, if conviction was necessary, that morbid adhesion did not only exist, but that it sometimes existed to a great extent; and that any degree of contraction, which the uterus could exert, would be unequal to the task of detaching it."—(Pract. Observat. p. 107.)

CASE V. *Partial adhesion of the Placenta, terminating fatally on the* 18*th day after delivery from successive attacks of hemorrhage.*—I was consulted in this case on the tenth day after delivery. The subject of it, a young lady of relaxed habit of body, but otherwise healthy, had borne a number of children in rapid succession after easy and natural labours. She had always been delivered by a midwife, the services of the ordinary medical attendant of the family being kept *in retentis*. On the present occasion, he was hurriedly sent for from an adjoining room to assist in the deliverance of the placenta, in consequence of an alarming flooding occurring suddenly after the expulsion of the child. The midwife, jealous of interference with her official duties, by incautious and forcible attempts to complete the third stage of labour, had broken the general mass of placenta, and left the ruptured part of its substance behind. My friend suspected, from the difficulties which she described she experienced in the removal of the secundines, and was quite satisfied after examining them, that a portion was disrupted. That portion was not within vaginal examination, and great objections were made on the part of the patient to the introduction of the hand *in utero*. No effort, therefore, was made to withdraw it, external compression and the application of vinegar and cold water having abated the hemorrhage. He remained two hours, and left her well. Next morning he learned that there had been a frequent recurrence of pain through the night, with discharges of fluid blood, and occasional expulsion of coagula. These symptoms, he informed me, had continued alternately to recur to a greater or less extent every day. The patient unfortunately laboured under pertussis, and the quantity of fluid discharge was much increased by the frequency and violence of the paroxysms of cough and spasmodic hoop.

When I saw her, considerable local uterine irritation, especially upon pressure, was experienced; but there was no abdominal tumefaction, and the discharge was not fetid or offensive. The general system had evidently suffered from loss of blood. The pulse was quick and small, and the countenance anxious and blanched. Though there was great prostration of strength, she was able, with a little assistance, to raise herself up in bed, and her spirits were cheerful.

As the case did not admit of manual means of relief, I only gave general directions to support the patient's strength by light nourishment. Acid tonics were recommended to be taken internally, and on the returns of flooding, which I earnestly advised to be watched, cold applications were to be applied externally. This latter symptom recurred almost every day preceded by slight after-pains, and, in spite of every means used to moderate it, completely exhausted her strength. On the eighteenth day after delivery, considerable uterine action supervened, with profuse hemorrhage. If any difference of opinion had ever existed as to the cause of the frequent uterine discharges, on the supposition that this might be a case of simple *menorrhagia lochialis*, all doubt on the point was removed by the expulsion of a portion of the placental mass, a few hours after the last discharge. She never rallied after this, but died on the following morning completely exsanguined, retaining her senses to the last.

I only saw the patient once, and got the preceding history from her medical attendant, who also informed me that the disrupted portion of the placenta was about the size of a large orange, and when expelled was not, what it usually or almost always is, in a state of putridity. In cases of premature expulsion of the fœtus (before the fifth month,) instances not unfrequently occur of the whole placenta remaining in the uterus for the space of a month, without undergoing the process of putrefaction. If the placenta in this case was in the state represented, the circumstance may be explained by supposing that the power which this mass possessed of resisting the putrefactive process, was derived from the influence it received in its adhesion to the uterus, and in the maternal circulation being carried on through its cellular structure, and that that adhesion gradually loosened by the slow process of absorption, more than by suppuration, was at last removed by uterine action.

Although no direct assistance could be afforded for the removal of the disrupted and adherent portion of placenta, which was quite beyond the reach of art when I was consulted, I have ever regretted that I did not suggest and urge transfusion as the only direct remedy left to supply the continued losses of blood. I know no description of cases in which Dr Blundell's application could apparently be more successfully employed than in the one under consideration. The practice, I fear, has not received the professional support it deserves, in consequence of experiments having been made in instances almost hopeless from sudden and profuse hemorrhage without uterine action, in place of fair trials of its advantages being instituted in cases where there were probable chances of recovery.

With Dr Ramsbotham's excellent observations on disruptions of the placenta I cordially and generally concur. I agree with him in thinking, that "the case, being the offspring of ignorance or mismanagement, ought never to happen, and will not occur under common caution."* But I cannot acquiesce in his remark which follows, "When it has actually happened, after the failure of the first attempt to remove the broken and still adherent portion, any farther manual assistance is inadmissible." Precepts leading to a "do-nothing" practice might be founded on such a remark. But, in this respect at least, I consider that the remark is incorrect, in so far as it is too general, and ought to have been guarded by a reference to time, after the occurrence of the accident. For example, in the present case, inasmuch as I was concerned, it was impossible to have introduced the hand into the uterine cavity ten days after delivery; but the operation can always be performed without much difficulty in ten hours after the occurrence of the accident, and, I conceive, ought to be attempted within twenty-four hours, in every case, when there is reason to suspect the existence of so fatal a cause of hemorrhage. "If you fail," says Blundell, "to get away the placenta, keep the case for four hours without flooding comes on; and by the end of that time you will find, that although you could not bring it away at the end of the first hour, you can easily do so on or before the fourth."—(Lectures, p. 160.)

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* Practical Observations, Part i. p. 166.

Although I dissent from this practice, and consider that when the hand is introduced within, it should not be withdrawn from the uterine cavity, until the extraction be completed, yet the advice now given corroborates the course I would pursue, were I called to such a case, after the lapse of four hours. It is conformable to experience, that some succeed, when others fail; and I believe it will be found, that those who succeed best, succeed from not being easily deterred; and that those who fail at last, generally fail, because they act at first from an irresolute purpose. It is to such a cause that the discharge of professional duty becomes so often a dead letter in performance. I imagine, at least, that too many are liable to relinquish, or rather to be over-persuaded by patients and their relations to abandon a decided line of procedure, in order to get quit of a disagreeable task, and for fear of inflicting a little temporary pain. The task I refer to (the introduction of the hand in utero) although it were accompanied with much greater suffering than occurs in the worst cases, ought to be carried through in opposition to all reluctance and entreaty from whatever quarter they may come, from fear of the ultimate and irremediable consequences ensuing from its neglect. The 12th, 13th, 14th, and 15th cases in White's treatise on Lying-in-Women, and the 137th and 138th cases recorded by Perfect, afford most melancholy illustrations of the fatal consequences in leaving to nature, what art can safely accomplish. The 139th case by the last author furnishes a most instructive lesson to those, whose practice is, when no bad symptoms appear, to leave the separation of the adherent placenta to the spontaneous contractions of the uterus, considering when the *os internum* becomes soon and closely contracted, that it is dangerous to attempt its dilatation, and after repeated essays, relinquish the operation from the difficulties which they meet with. The case cited by Burns should ever be present in their mind. "I have known," says he, "the placenta retained for four days, by an adhesion not larger than a shilling. This case proved fatal by loss of blood, which continued to take place, I understand, in variable quantity, during the whole time. No attempts were made to relieve the woman, until she was dying."—(Principles of Midwifery, p. 512.) These ought far more to be dreaded and apprehended than the ideal dangers of the operation itself—an operation, although it even were unnecessarily had recourse to, can always be safely and harmlessly performed; and when necessary, becomes not only a useful measure, but an indispensable expedient for the patient's recovery. The melancholy result of this case must apologise for these digressive remarks.

Capuron and Gardien recommend the same practice which I have followed, when the placenta is morbidly adherent. I have already answered one of the objections, which have been urged against the method of extraction, by insinuating the hand between the uterine surface and that of the placenta, and peeling the latter off, and proved by the result of the cases that " the hazard of exciting inflammation on the surface of the uterus by the pressure of the fingers" is more ideal than real. The other objection—" the great risk of lacerating the substance of the uterus by tearing off a part of the placenta literally blended with it,"—appears to me to be raised more on the fear of the operation than founded on its results. With ordinary care and caution, such an accident can never happen. On the contrary, laceration in the unstretched state of the uterine parietes after the expulsion of the child would require, I am convinced, much greater manual force than is imagined, or could easily be applied. If we consider also the condition of the parts when adhesion takes place, such an injury is much more unlikely to be inflicted, for if my opinion be correct, that the connection between the uterus and placenta is generally the effect of chronic metritis, it will be found that the inflammatory action, in place of causing softening degeneration of texture, will have rendered the structure of the uterus more firm and dense. The only exception to this mode of operating, exists in those very rare instances in which adhesion has been effected by means of cartilaginous and osseous change of structure, when artificial assistance might perhaps be dispensed with, and the separation of the indurated parts left to the natural efforts.

I must qualify my remarks by limiting their application to such cases only where osseous cohesion has taken place between the uterine and placental surfaces. I know not, if there ever existed, between mucous membranes, such a medium of connection. If, as I am inclined to believe, osseous degeneration of structure be a disease more peculiar to the placenta than the gravid uterus, it is obvious, that the removal of so hard and incompressible a body, by preventing uterine contraction, becomes the more necessary. When pregnancy is complicated with fibrous tumour in the uterus, that morbid growth might possibly be mistaken for an adherent placenta. Madame Boivin describes a case in which the hemorrhage was erroneously attributed to the insertion of the placenta upon the cervico-uterine orifice, but on dissection of the patient, who died on the eleventh day after labour, " there was found, on the right side of the uterus, a tumour, about three inches and a-half in diameter, of firm tissue, of a bluish-black colour, weighing twelve ounces and a-half; it was continuous with the substance of the uterus."—(Practical Treatise by Heming, p. 191.)

I have been induced to insist the more on the necessity of removing every portion of adherent placenta, as I believe it is almost always practical to do so without danger, that the practice has been opposed and condemned by such influential authorities as Professors Hamilton and Davies. The former says, " he has attended many cases where two, three, or more days have intervened between the birth of the infant and the separation of the adherent indurated portion of the secundines, and he never witnessed any untoward symptom, such as flooding or subsequent irritative fever. In one case, a mass weighing eight ounces, was retained five days without occasioning any symptoms indicating danger." (Hamilton's Practical Observations, Part i. p. 208.) This case only proves that a large portion of adherent placental mass can sometimes be left with safety *in utero*, and there are many instances upon record terminating in the same favourable way. But there is an opposing evidence to the general happy result attending the ultimate expulsion of adherent portions of placenta, furnished by the 5th case. The question, therefore, is, as it is not always safe to trust to nature to perform the operation, whether there be less danger to the patient, which I apprehend there is, in accomplishing it by artificial means. For this reason, I object to the following advice given by Dr Davies: " The proper, and indeed the only safe practice to be adopted," he says, " in such cases, is to withdraw, by careful detachment, all of the placenta, that is found not morbidly adherent to the uterine parietes, leaving the diseased remainder to such kindly offices of nature, as she may be competent to exert for their expulsion."* What the offices of nature takes to remove in a few days, the offices of art would, I apprehend, be able to effect in thrice as many minutes; for indurations separated in so short a period by sloughing or suppuration appear not to be of a firm or unmanageable character.

Dr Burns seems to coincide in opinion with Drs Hamilton and Davies. He says " adhesion may occur with or without change of structure; but in general the structure is more or lesss altered, the adhering part being denser than usual, and sometimes almost like cartilage. The separation of the adhering portion should not be hastily attempted, nor by insinuating the fingers between it and the uterine surface. It is better to press on the surface of the placenta, so as thus to excite the uterine fibres to contract more briskly at the spot; or, by gently rubbing, or it were, pinching up the placenta between the fingers and thumb it may be separated. If, however, the adhesion of the part of the placenta be very intimate, we must not, in order to destroy it, scrape and irritate the surface of the uterus, but ought rather to remove all that does not adhere intimately, leaving the rest to be separated by nature." (Principles of Midwifery, p. 373.)

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* Obstetric Medicine, p. 1063, Vol. ii. Barlow speaks to the same effect; see Essays on Surgery and Midwifery, p. 258.

Dr Ramsbotham has proved demonstratively by cases 15, 18, and 19, the utter inability of the uterus to detach the placenta, under such circumstances of morbid adhesion, and the necessity of the introduction of the hand for its manual separation. The uterus had contracted upon the placenta to its greatest extent, without being able to throw it off; nay, the greater the degree of contraction, the greater the improbability of its natural exclusion. Dr Burns's remarks are judicious, in so far as they condemn all impatience in the operation, and recommend caution and gentleness. But they fall short in not recommending such an extension of patience and manual assistance as are necessary to complete the detachment. The care and caution required in the extraction of the placenta is necessary not only for fear, as he thinks, of uterine disorder being excited, but, as I conceive, lest hemorrhage ensue from the manipulations not having been carried to a sufficient extent, and uterine or constitutional affections arise from undetached portions of placenta having been left behind, the unavoidable consequence of the operative means not being prolonged, so as to effect a total separation.

Dr Ramsbotham's experience corresponds with my own regarding the causes of placental adhesion. "I have observed," says he, "adhesion of the placenta to follow a blow, pressure, fall, or other external injury on the belly during the latter stage of pregnancy." "But every blow, or other external injury," he adds, "may not be productive of this mischief, since so extensive a space of uterine surface is free from placental attachment." (Practical Observat. Vol. i. p. 76.) Inflammation of the uterus, when once excited by an external injury, may extend beyond the locality of the injured spot; and the surface occupied by the placental attachment, although at first beyond the sphere of the accident, may afterwards be involved in the progression of the diseased action. It is only in this way that morbid placental adhesions to the posterior uterine wall can be explained, unless we suppose that similar effects are produced by a diseased action, spontaneously established in the uterine vessels furnishing the placenta, or in that of the deciduous membrane connecting it to the uterus, or that it has originated in the tissue of the placenta itself, or its membranes, and spread from these into the uterus. Undoubtedly cases of idiopathic chronic metritis, and placental disease occur, occasioning adhesion between the uterine and placental surfaces; but they are, I suspect, of comparatively rare occurrence to those instances in which the morbid action has been induced by, and can be traced to external causes.

The existence of the morbid adhesion during pregnancy is a point, however, from its practical bearings, of more consequence to determine than either its site, origin, or cause. Although, probably, chronic *metritis* or *placentitis*, under any form, might not by any treatment be controlled, so that the morbid products upon which adhesion depended could be prevented, yet the knowledge or suspicion of such a termination of the morbid action would lead to a more efficient and decided line of proceeding, after the expulsion of the child, in reference to the management of the placenta. When the placenta is wholly adherent, no danger from hemorrhage can ensue, and an hour after delivery can safely be allowed to elapse before the hand be introduced for its extraction; but, in cases of partial adhesion, a very short space of time lost before having recourse to manual means of assistance may become a positive source of injury or danger to the patient's life.

"It cannot, therefore, be too deeply impressed upon the mind of every person engaged in the practice of midwifery, that the effect of the loss of blood after the birth of the infant, whatever may be its quantity, must always be uncertain,—and that it may rapidly sink the living powers in some individuals, while in others it may give such a shock to the constitution, as shall occasion a broken state of health."—(Hamilton's Practical Observations, Part i. p. 276.)

The archives of obstetric medicine are full of instructive warning, and the private annals of every accoucheur in extensive practice will bear me out, regarding the sudden and fatal termination of life in cases of partially adherent placenta. A case of sudden fatality (within two hours after delivery) is recorded by Ramsbotham, in which only about half an inch of the placental surface was detached, the remaining part being wholly adherent throughout its whole extent. When a large portion is detached, death, I have known to occur, within half an hour after the expulsion of the child, and, according to Gooch, in five or ten minutes. Two cases of this description have come under my knowledge; the one in a thirteenth, and the other in a first labour.

My brother, Alexander, (who practised with me,) was called to the first case. He arrived within an hour after the birth of the child, and fifteen minutes after the death of the mother. She was a healthy woman, and had been delivered out of bed by one of her neighbours, who made no attempts to remove the placenta, which, on opening the body afterwards, he found morbidly adherent to a considerable extent at the *fundus uteri*.

The result of the other case was equally melancholy. The patient had rather a severe and lingering labour, and was attended by a midwife, who, in consequence of the death of a relation, was anxious to be relieved from her attendance on the case, and had, for the purpose of quickening labour, delivered the patient on her knees out of bed, and in that position, through hurry to complete the process of delivery, brought away by mere force a large portion of placenta with the funis. An alarming hemorrhage came on, and before the unfortunate victim could be removed to bed, she fell down upon the floor, and ***amisit cum sanguine vitam.*** I arrived about a quarter of an hour after this catastrophe. There had been a prodigious discharge of fluid blood, and the unwashed state of the floor, on which she died, gave visible and fearful evidence of its extent and fatality. On inspection after death, I found the uterus large, empty, and flaccid. The adhesion of the attached portion of placenta was at the fundus, a little to the anterior parietes of the uterus, and might occupy, as nearly as I could guess, about a third of its whole surface. In detaching the united surfaces, I plainly perceived, that their connection, although very intimate, could be separated by the fingers, without the uterine structure being exposed to the slightest risk of injury, if the head guides, when the eye cannot direct the hand, in the operation. The patient was a healthy robust person, about 22 years of age. Both women complained for many months of a dull dragging pain in the uterine region, which was much increased by walking up stairs,—and for relief from which, they were obliged to lie during night on the fore-part of the belly.

It occurs to me, therefore, in cases of suspected placental adhesion, as its extent can never at first be ascertained, that the hand should immediately be passed *in utero*, after the termination of the second stage of labour, and before hemorrhage has supervened, or any other symptom has arisen to indicate the necessity of such a measure. The presence of the hand is not for the purpose of removing at once the placenta, but is to be retained in order to act as a uterine irritant at first, and as a necessary auxiliary afterwards in completing the separation, which uterine contraction is unable further to effect. But it ought never to be forgotten, that its introduction is to serve a twofold purpose, the first half of which is not the extraction of the placenta, but the contraction of the uterus, and that it is unsafe to invert this order. When a practitioner is thus put on his guard, "being forewarned he is forearmed." I need scarcely add, that all the other means for exciting and keeping up the contractile power of the uterus should be rigorously adopted and pursued, before alarming symptoms of hemorrhage manifest themselves. The great danger of sudden death from profuse loss of blood in cases of partial placental adhesion's, imperatively calling for the earliest interference of art, naturally demands our early attention into the symptomatology of the morbid action.

On this part of the subject, the writings of the early authors on midwifery throw no light. Of cases recorded by them, " in which the placenta was with difficulty delivered," in consequence of some " parts of it adhering to the womb, being hard, indurated, or scirrhous," and requiring manual extraction, there is no lack; but in vain we seek in their histories for information regarding the causes in which they originated, or the symptoms which indicated their existence. The almost total silence on these important points forcibly demonstrates the little regard, in which the obstetric art was long held as a branch of medical research. Even still the knowledge we possess, in reference to the locality of uterine pain, for a longer or shorter period preceding parturition, as a symptom of morbid placental adhesion, is exceedingly limited and imperfect. Dr Hamilton indeed denies, that any index denoting such an occurrence exists. He says, " that he has met with many cases of adherent placenta, where there had been no previous pain in the side; and that, on the other hand, in several cases, where the patients had been in much alarm in consequence of having experienced the pain alluded to, there proved to be no morbid adhesion."—(Pract. Observ. Part i. p. 288.) In opposition to this, Blundell affirms, " when this disease exists," (placental adhesion from inflammation in the early months), " on questioning the patient, she tells you, she has had pain and uneasiness in the part during the early, or the whole period of gestation."—(Lectures p. 150.) To the same effect is the testimony of Dr Ramsbotham. Many of his cases on this head are very instructive, and his opinion is the more valuable, that it is founded upon evidence drawn from personal experience.

The description of the situation of the pain, as well as of the kind of pain, is often very vaguely and inaccurately given by the patients. The test of discrimination proposed by Dr Ramsbotham should uniformly be applied. But it is only by minute and particular inquiry into all the circumstances of each case, that it is possible to arrive at any thing like a correct diagnosis, which, I acknowledge, it is sometimes very difficult to obtain, between pain in the uterine region, more or less constant and of a heavy dragging nature, as it is usually expressed, as indicating placental adhesion, and those uneasy sensations and painful affections of the uterus, in which the pain is more diffused over, and shooting about the abdomen, dependent on increased sensibility of that organ, and that numerous train of annoying and distressing pains, to which pregnant women are liable, connected with spasm of the abdominal muscles, and functional derangement of the large intestines. The subject requires and deserves patient and attentive investigation, from the importance of the practical application to which such an inquiry leads. That pain should exist as a symptom of placental adhesion, we would be led to infer by merely reasoning from analogy, if it be admitted, what indeed cannot be denied, that "the uterus when locally inflamed becomes adherent to the placental mass, by the intermediation of an extraordinary vascular tissue, which it is well known adhesive inflammation generates. The inflammation is generally occasioned by accidental or external causes, but it may possibly arise as an idiopathic disease; and it appears not improbable, that the differences of the kind of pain complained of, may be all referable to, and accounted for, by the greater or lesser degree of intensity of the inflammatory symptoms, modified by the nature of the exciting causes, and the peculiarity of individual constitution, and by the products to which the morbid action gave rise, exemplified in the extent of the adhesions and general enlargement of the placental mass, and possibly also by the site of the morbid action itself, whether it originated in the placenta or in the uterine parietes.

The practice recommended by Dr Burns when uterine hemorrhage is apprehended, is so applicable to cases of suspected placental adhesion, that I cannot conclude this paper better than by quoting his excellent remarks. "When a woman," he says, "is known to be subject to hemorrhage, we should give her a full dose of laudanum immediately after delivery, excite the action of the uterus by external pressure or friction; and on the first appearance of discharge, perhaps in most of such cases, whenever the child is born, we ought to introduce the hand into the uterus." (Principles, 508.) So should the practice be with supposed placental adhesion. The introduction of the hand is the first necessary step; and it can always be easily done, before either swelling of the vaginal tunics, or the sudden spasmodic contraction, to which the cervico-uterine* orifice is so liable, have occurred. The next object is to excite regular, and prevent spasmodic, contraction of the womb. The separation of the placenta is afterwards to be effected. However tedious that process, and lengthened the time may be which is necessary for its completion, and I have shown that an hour sometimes is scarcely sufficient for that purpose, the hand ought not to be permanently withdrawn from its cavity, until the object for which it was introduced be fully attained.

Penicuick, 24th January 1839.

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* On this subject, see White's Treatise on the Management of Pregnant Women, p. 84. Perfect's Cases in Midwifery, Vol. i. p. 91, &c. Dr W. Johnson's Treatise, p. 206; and Giffard's Cases in Midwifery, 34, 72, 74, 92, 127, 134, &c.

ART. IX.—*Case of Disease of the Bladder following an Injury. Presence of Carbonate of Ammonia in the Urine.* By CHARLES RANSFORD, M. D. Fellow of the Royal College of Physicians, and Physician to the Royal Public Dispensary, &c. &c. (Read before the Medico-Chirurgical Society of Edinburgh.)

ADAM BEGG, aged 80, had been for some months affected with numbness in his legs, attributed to old age and debility. On the night of Wednesday, April 23, 1834, in attempting to get out of bed, (raised scarcely two feet from the floor,) being, as he says, perfectly sober, he fell, but knew not upon what part of his body,—becoming for a short time insensible. The lesions perceptible were contusions on the right thigh and groin; the limb shortened, and the foot everted. He was unable to move either limb, or to turn himself in bed. I saw him first on Saturday, May 3d, and learnt that, since the accident, he had lain constantly upon the back, and passed urine in small quantities, accompanied with much irritation, the day after the injury. He did not suffer from complete retention until April 30th, when he sent for a surgeon in the neighbourhood at midnight. The contents of the bladder were drawn off, affording great relief. When I visited him three days afterwards, I found him suffering from retention, no urine having been passed or drawn off since the introduction of the catheter three days previously. Notwithstanding this, there was no distension of the bladder. The bowels had been freely opened in the morning by castor oil; there was a purulent discharge from the glans, and phimosis also existed; his general health appeared good. His requests to have the catheter passed were so urgent, that I introduced it immediately. Not more than twelve ounces of urine, of a dark-red colour, as if mixed with blood, were taken away, not in a continuous stream, but in jets, at intervals, assisted by the action of the abdominal muscles. He had perfect command over the *sphincter ani*, and did not complain of tenesmus. Six leeches were directed to be immediately applied to the right groin, and afterwards warm fomentations; and he was ordered to have at bed-time ten grains of Dover's powder, and one ounce of castor oil next morning.

May 5th, Bowels freely opened; the bladder is very irritable; some drops of urine escaped at intervals. The bladder had been twice emptied by the catheter on the preceding day, with temporary relief. No mucus nor deposit of any kind was at this time perceptible; and upon testing the urine with litmus-paper, it was found to be slightly acid. He complained of great pain about six inches down the urethra, aggravated by the introduction of the catheter, which suffered at this spot slight obstruction, overcome, however, without any force being necessary, but not of any, on pressure over the pubes. Five grains of the soap and opium pill were ordered every four hours, and mucilaginous drinks to be freely given.

May 6th, Considerable relief was experienced after taking one pill; slept four hours; the pain, however, returned, and no further benefit resulted from the repetition of the opiate. The pulse was quick; the tongue furred; the skin hot and dry; and the bowels confined. The urine drawn off yesterday was of a dark colour, and turbid, not decidedly bloody. He was ordered immediately to have an ounce of castor oil, and a pill consisting of one grain of calomel, one grain of ipecacuan, and half a grain of opium, was ordered immediately, and to be repeated at the end of six hours. Blood was also directed to be drawn from the loins by cupping.

May 7th, 9 o'clock P. M. On account of his position, the cupping was performed with great difficulty; only four ounces of blood were taken. He had passed urine since last evening, but spent a very restless night. The urine was dribbling incessantly into a tin vessel kept constantly applied. He was sweating profusely; with the pulse at 100; and he had rigors about six o'clock. The pill of calomel, ipecacuan, and opium was directed to be repeated.

May 8th, Mr Syme saw the patient with me. He had slept well after taking one pill. Febrile symptoms were much abated; the tongue moist; and the pain and irritability of the bladder considerably diminished. Mr Syme carefully examined the bladder, and drew off about six ounces of turbid, dark-coloured urine still acid, and a small quantity had been passed naturally before the visit. He was ordered immediately half an ounce of castor oil; and the pill of calomel, ipecacuan, and opium, to be repeated.

May 11, Appeared much sunk; complained of constant pain about the perinæum and pubes; pulse 80; tongue dry; urine dribbling away constantly. Leeches were applied to the perinæum. The powder of *Arbutus uva ursi*, with carbonate of potass, was ordered, with extract of hyoscyamus at bed-time, and an anodyne injection if necessary.

May 15th, Much better, but still suffers from pain in perinæum and pubes. Urine comes away involuntarily, and exhibits a muco-purulent deposit. It is decidedly alkaline, having a very strong smell of carbonate of ammonia, though by no means offensive. The following analysis was made at this period by my friend, Mr Russell, under the superintendence of Dr Gregory.

" *Analysis of urine.*—We first filtered four ounces, then added to the filtered liquor muriate of lime so long as any precipitate appeared. The muriatic acid combined with the ammonia, existing in the urine in the state of carbonate, and the lime with the carbonic acid, forming the insoluble carbonate. The carbonate on being collected and dried weighed 50 grains carbonic acid, 22 × lime 28, but 22 of carbonic acid combine with 17 of ammonia which might be thus expressed;

$\overset{6}{\text{C.}} + \overset{16}{2\ \text{Ox.}} + \overset{3}{3\ \text{H.}} + \overset{14}{\text{Nit.}}$ subtracting an atom of water, there remains
$\overset{6}{\text{C.}} + \overset{8}{\text{Ox.}} + \overset{2}{2\ \text{H.}} + \overset{14}{\text{U.}} = 33$ grains of urea.
This is nearly 2 per cent. the usual quantity found in healthy urine."

May 16th, On account of the typhoid symptoms now present, the sulphate of quinine and diluted sulphuric acid, with wine, and full opiates by the rectum were administered. He continued nearly in this state until June 1st, the opiates failing to procure sleep. The diluted sulphuric acid was regularly taken three times in the day; but though the carbonate of ammonia disappeared, the urine continued alkaline; and the quantity of mucus deposited was much increased, with the addition of the earthy phosphates. In the evening of June 1st, a sudden accession of pain took place, and infiltration of urine into the scrotum, and gangrene rapidly succeeded. He died within eighteen hours afterwards.

Post mortem appearances forty-two hours after death.—The bladder contained about eight ounces of urine mixed with mucus; its cavity was much diminished by the coats being nearly four times thicker than usual; the rugæ upon the inner surface were strongly marked; the triangular pouches between them were deep and well-defined. The mucous membrane was highly vascular, and softened at the prostatic portion only. The prostate was healthy, not containing any calculi or sabulous matters. The parts in the neighbourhood of the infiltration, particularly the coats of the bladder, were much softened. The scrotum and penis were in a state of gangrene. The former with the testes had decomposed so rapidly after death, that a rupture had taken place, and urine mixed with blood escaped on the bed. From this circumstance, we could not discover the ulceration by which the urine had escaped into the scrotum. The right kidney was softer than usual, and some congestion was observed in the cortical part. The left kidney was natural.

I was informed by his son, that he had been occasionally subject to irritability of the bladder, after any excess in drinking, in which he sometimes, though not frequently, indulged. The patient positively assured me that he never had suffered from syphilis or gonorrhœa. On examining the right hip-joint, we found the neck of the femur fractured in two places. One fracture extended through the trochanter, the other round the head of the bone. Upon the capsular ligament was some osseous deposition. But there was no attempt at reunion.

That carbonate of ammonia was occasionally secreted by the kidneys, was first presumed, I believe, by Nysten, who relates a case of ascites, in which the urine contained carbonate of ammonia, but not a trace of urea.* Dr Graves announced the same facts some years since, in two cases under his care. In one this carbonate was discovered in the *recently* voided urine of a fever patient, in whom, as the health improved, the ammonia disappeared. In the second case by Dr Graves symptoms of anasarca and intestinal inflammation, attended with deposition of the earthy phosphates, terminated fatally. The urine was perfectly limpid when voided, and had not the slightest putrescent smell. In both of these cases the salt was found in urine passed within half an hour and two hours after the bladder had been completely emptied. In this as in all the instances in which carbonate of ammonia has been present, no traces of urea could be discovered. The kidneys, as the right one in Begg's case, were found to be enlarged and turgid with blood, but the mucous membrane of the bladder was healthy. In two cases of *diabetes mellitus*, treated in the Royal Infirmary of Glasgow, and the particulars of which are well described by Mr M'Gregor in the London Medical Gazette, (Vol. xx. p. 268,) during the course of the disease, and whilst opium was exhibited to the extent of three grains daily, the urine became strongly alkaline, containing carbonate of ammonia but no urea ; the specific gravity of the urine of one of these patients is mentioned 1.044. After a week the carbonate of ammonia was replaced by urea.

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* Nysten, Recherches de Chimie et de Physiologie Pathologiques. 8vo. Paris, 1811, quoted by Willis on Urinary Diseases and their treatment. London, 1838.

My friend Dr Irvine, when acting as clinical clerk in the Royal Infirmary of this city, during the winter of 1833, observed the same secretion of carbonate of ammonia in the urine of several patients suffering from continued fever of a typhoid nature, and likewise in one case of dropsy. The urine in which the salt was present, on being treated with any diluted acid, instantly effervesced briskly. In the case of dropsy the urine was of a light nut-brown colour, of specific gravity 1020. Litmus paper previously reddened being introduced, indicated an alkaline re-action; it had a strong ammoniacal smell, and effervesced with acids. On being boiled, and a rod dipped in muriatic acid held in contact with the vapour, copious white fumes were produced, indicating the presence of ammonia. On treating the urine with muriatic acid after boiling, no effervescence took place. In this urine no sediment was observable, and on treating that liquid in the usual way, in order to precipitate the triple phosphate, viz. by the addition of a solution of ammonia, none of this substance was thrown down. A portion of this urine examined in the usual way, in order to ascertain whether it contained any urea, gave no indication of the presence of that substance.

Dr Irvine kindly favoured me with the particulars of one of the fever cases. In this urine a dark-coloured sediment was perceptible, it effervesced strongly with acids, and had a distinct alkaline reaction on litmus-paper. The gas disengaged during the addition of acids was the carbonic acid. On passing a stream of it through lime-water, a thin pellicle was found on its surface. The urine was boiled, and a slip of litmus-paper, moistened and previously reddened by a weak acid, was held in contact with the vapour, and indicated the presence of an alkali; a rod dipped in muriatic acid, and held over the boiling fluid, instantly detected the presence of ammonia, forming with the vapour dense fumes of muriate of ammonia. After boiling, the introduction of muriatic acid into the urine caused no effervescence; a portion of urine set apart and treated with a solution of ammonia gave no trace of the triple phosphate. Another portion was examined in the usual way for urea, but was found not to contain any.

The labours of many eminent chemists and pathologists have established beyond dispute the fact, that material changes take place in the fluids of the body during disease. The subject is daily increasing in interest, and when we take a retrospect of all that has been already effected, it is not unreasonable to anticipate the most important results from the cultivation of this department of chemistry. Amongst so many celebrated works on the chemical composition of the urine, there is very slight mention of the subject which comes under our notice in these cases. Dr Willis, in his lately published valuable work on urinary diseases, has collected all the information which he believes we possess, on the presence of carbonate of ammonia in the urine of persons labouring under disease. He combats the opinion entertained by some of his predecessors, that the presence of this salt is owing to the decomposition of urea in the bladder. In those cases of diseased prostate, and of inflammation of the mucous membrane of the bladder, in which the urine is ammoniacal, it is highly offensive, and is most probably due to the rapid decomposition of the urea during its sojourn in the bladder. From the facts which I have now related, there does not appear to me room for hesitation that it is in the kidneys that the change takes place; and it does not appear so remarkable when we consider the chemical constitution of urea and carbonate of ammonia respectively,

$$\text{Urea consists of}\left\{\begin{array}{ll}\text{Carbon,} & 1\text{ atom,}\\ \text{Hydrogen,} & 2\\ \text{Oxygen,} & 1\\ \text{Azote,} & 1\end{array}\right\}\begin{array}{c} .\quad .\\ .\quad .\quad .\quad .\\ +\ 1\text{ atom water} =\\ .\quad .\quad .\end{array}\begin{array}{ll}\text{Carbon,} & 1\text{ atom.}\\ \text{Hydrogen,} & 3\\ \text{Oxygen,} & 2\\ \text{Nitrogen,} & 1\end{array}$$

which makes 1 atom carbonic acid and + atom ammonia, = 1 atom carbonate of ammonia.

We cannot explain the reason why, instead of urea, carbonate of ammonia should be formed, and can only refer the change to the general disorder of all the functions which takes place during the febrile state, which is evident from the vitiated and deficient secretions. The digestive organs are especially at fault, their functions with those of assimilation being much injured.

November 1838.

PART II.

CRITICAL ANALYSIS.

Art. I.—*A Treatise on Inflammation.* By James Macartney, M. D. F. R. S., &c. &c. 4to. London, 1838.

From the earliest period of our professional recollection, the nature of inflammation has formed a fruitful source of discussion, and an important object of inquiry. Sthenic and asthenic diathesis, direct and indirect debility, increased and diminished action, the doctrines of Cullen, Brown, Darwin, and Hunter, have all in their turns excited attention and interest, and had their supporters and adherents. The great work of John Hunter, followed by the accurate investigations of numerous pathologists of the present day, have done much to settle many of the more prominent and important practical parts of the subject; but still there are various difficulties to be surmounted before we can thoroughly understand the doctrine of inflammation, and the whole of its bearings and relations. We have long understood that Dr Macartney entertains some peculiar views in relation to that part of the subject which has been supposed to be best understood, we mean adhesive inflammation; and are happy in finding him, by the publication of the present work, the expounder of his own opinions, instead of trusting to the imperfect accounts which have been communicated of them to the public through a foreign medium. He states, indeed, that the doctrines which he here promulgates through the press were many years taught by him in his lectures on surgery. But now that he has ceased to lecture, by having resigned his professorship in the University of Dublin, he feels it right to make known his opinions on inflammation generally, by an express publication; for though he considers the practice which is deducible from his doctrines, as having been long too well established to admit of controversy, he is doubtful as to the sentiments which the profession may form of the doctrines themselves, and is therefore very properly desirous that they should be fully understood.

In the first chapter of his work, the author treats of the *History of Inflammation.*

No such phenomenon exists in the vegetable world as inflammation in the animal; for in plants the process of reparation after injury takes place from the regular and natural growth of the vegetable body. The formation of galls, and excrescences of exuberant growth, bear, however, some degree of analogy to it. In the lower animals, where there are no visible nerves, inflammation does not occur; but the renewal of organs takes place, and even the multiplication of separate individuals, after incision and division, in a manner somewhat similar to what is the case in the grafting of plants. *Polyps* and other *gemmiparous* animals afford examples of this description. In *worms*, when divided, the cut extremities soon heal; but the author has only found that the anterior half reproduced the perfect worm. The *Planariæ*, the *Nereides*, and the *Nais variegata*, form with each section, a distinct individual. *Insects* are not susceptible of inflammation. The poison of *spiders*, when it does not instantly kill, stupefies. *Crabs* and *lobsters* reproduce their lost claws, from a sort of bud or gem, which is formed after the broken surfaces heal, which they do without any appearance of inflammation. The *Arachnida*, when young, will reproduce their limbs, but not when fully grown; which is the case also with most other insects. The *Mollusca* do not admit of genuine inflammation. *Oysters* and *mussels* may have a portion of their respective substances in a state of putrefaction, and yet still adherent to the sound part. In these animals, a foreign body, a piece of wire, will produce an exuberant growth of shell; and the presence of a small worm between the membranes, and the consequent deposition of ova, gives rise to the formation of pearls. The head of the *snail* is renewed, provided the ganglion, which lies above the œsophagus, and is analogous to the brain, has not been injured. In *fish* wounds do not inflame, and nothing like suppuration takes place in them after injuries. The tracts of the wounds made by introduction of wire under the skin and muscles are pale and smooth, and only moistened with a serous fluid. The reproductive power of fish is confined to their fins. In *reptiles* the author has seen nothing like appearances of inflammation. A *serpent's* tail is not reproduced; but the wound, on its removal, speedily cicatrizes. *Lizards* will regain their tails, but without the vertebræ. *Salamanders* are said to have much power of reproduction; and the author has seen attempts at renewal of the fingers and toes of the *toad* and *frog*. A portion of the skull of the former was supplied by half cartilage, half bone; but a depression continued in the place from which a portion of brain had been removed.

Birds are susceptible of inflammation from mechanical injury, but rarely from internal causes; *quadrupeds*, from both external and internal causes; but in the latter there seems to be little constitutional sympathy with local disease; for a *dog* or *horse* will continue to eat, though with severe suffering from accident or disease.

The human being is, above all others, susceptible both of internal and external inflammation; and from the complicated nature of its nervous system, local and constitutional affections sympathize in a high degree with each other. When, in man, we succeed in removing the inflammation produced by injuries, the reparative process goes in a manner similar to what takes place in the inferior animals.

The view given by the author of the effects of injuries in different classes of animals, impresses him with a conviction, that the power of reparation is in proportion to the indisposition or incapacity for inflammation; and that, therefore, "inflammation is so far from being necessary to the reparation of parts, that in proportion as it exists, the latter is impeded, retarded, or prevented, and that when inflammation does not exist, the reparative power is equivalent to the original tendency to produce and maintain organic form and structure; that it then becomes a natural function, like the growth of the individual or the reproduction of the species."—P. 6.

Mr Hunter, the author states, was aware of the possibility of wounds healing without inflammatory action, and would, he thinks, if he had lived, have modified very much his sentiments in relation to the phenomena of inflammation. The terms which Dr Macartney considers him as having "unhappily employed, of adhesive, suppurative, and ulcerative inflammation," and the views which he favours of this process being within certain limits a "sanative action," have misled, he thinks, many of his readers, and have induced them to consider Mr Hunter "as an authority for opinions directly at variance with those principles of philosophy, which it was the great object of his life to establish."

We do not, however, agree with the author, in thinking that there were any peculiar principles of philosophy except those of deduction generally, which Mr Hunter's labours, numerous and diversified as they were, were calculated to keep in view; nor are we of opinion, greatly as we estimate the merits and originality of our distinguished countryman, that it is likely he would ever have been able to reconcile the various discrepancies which occasionally appear in his works; or to develope sufficiently the consequences of many of his facts, amid the multifarious objects to which his attention was continually directed. He has done a great deal, much more than is likely again to be done by any one man: but human powers are limited; and it was hardly possible that he should be able to follow out the whole of the analogies which presented themselves to him, or to reconcile or remove the difficulties and contrarieties which could not but occur in subjects of physiology and pathology, where there are such numerous sources of error. With those impressions, therefore, relative to the high merits of Mr Hunter, we cannot view his works as oracles, which, if properly studied and understood, will be adequate fully to explain every thing of which he treats, but as a "rich and deep mine" of physiological and pathological science, abounding in important facts, and in interesting and ingenious reasonings.

The author next goes on to the consideration of the *Phenomena of Inflammation*. Besides *Heat*, *Redness*, *Tumour*, and *Pain*, the author is of opinion, that *alteration* or *suspension of the natural secretions* of the part should be regarded as necessary attendants of inflammation.

With regard to heat in external inflammation, it does not exceed the interior temperature of the body, though it does so in inflammation of internal organs. The generation of heat in the animal body, the author regards as being so little understood, as to make it impracticable to give a satisfactory account of its production in inflamed parts. He does not regard it as dependent on respiration; for vital temperature is preserved when the function of respiration has not commenced, as in the fœtus: but in this, a lower temperature exists than is possessed by the parent, though the heat of the mother's body is acquired after death. If respiration were the cause of animal temperature, the author thinks there ought to be a limit to its generation; but he found that a cluster of the larvæ of the *Musca pendula*, inclosed in a mass of ice, reached 32° (the external air being 30°) without freezing. Respiration was now completely suspended; and if there were a constant evolution of caloric, it must have been very large, and far exceeding what could exist in such small creatures in a latent state.

Animal heat he is disposed to consider as one of the phenomena of sensibility. It increases with excitation of the vital powers, and is diminished by the reverse. During the swarming of bees, the heat is greatly increased; and in incubation, the temperature of the hen's breast rises to 104°.

Abstraction of nervous influence will diminish temperature, as in cholera; but division of the large nerve of an extremity, or of the spinal marrow, will increase it; and those apparent contradictions, the author thinks, are reconciled by the circumstance of "exciting passions or instincts acting on organic sensibility;" while in the case of the division of nerves, or of the spinal marrow, "the organic actions are liberated from the restraints under which they are carried on, as long as the integrity of the nervous system is preserved." To the cessation of individual life, and freedom from the restraints of the nervous system, he attributes the occurrence of vermicular motion in the intestines, and of convulsions in the voluntary muscles after death.

Of *redness*, *tumour*, and *pain*, he considers the last as the most difficultly explicable; for though various mechanical causes, as tension and pressure, may increase, these cannot produce it. He is disposed to think that the additional number of minute vessels carrying red blood, may give rise to the phenomenon; though we cannot but consider this idea as contradicted by many of the examples to which he has occasion to refer, as blushing, the turgescence of the erectile tissues, and the rapid growth of the horns of the deer kind. He mentions the various descriptions of pain, as dull, acute, scalding, tension, aching, burning, and lancinating, and considers them as dependent partly on the character of the inflammation, and partly on the structure of the parts involved in it.

The changes which take place in the character of the *secretions*, in inflammation, are various. They are increased in slight inflammations; but not only increased, but much altered in their character when this is greater: becoming thicker or purulent, the serum of blisters coagulated, and cutaneous secretions of various kinds rendered glutinous and adhesive. When inflammations are excessive, secretion of every kind is stopped.

The *consequences of Inflammation* come next under the author's notice. These are to be carefully distinguished from the processes in which reparation of injuries is to be effected, and comprise chemosis, œdema, vesication, suppuration, and the total disorganization and death of the part.

Chemosis, the author states to be extravasation of blood; but in the employment of this term he deviates from common usage; for while *ecchymosis* is employed to designate effusion of blood, *chemosis* is applicable alone to a well-known disease of the eye, and is derived from a wholly different root. Extravasation, the author considers as always arising from ruptured vessels in inflammation; and the laxness of the cellular texture, when this circumstance most frequently takes place, is favourable to its occurrence. *Œdema*, or diffused extravasation of serum, is generally the effect of laxity of fibrin, and is an unfavourable occurrence. *Vesication*, or the elevation of the cuticle from the new skin, by an effusion of serum, may either arise from irritation of the skin, or from the occurrence of inflammation in constitutions indisposed or incompetent to effect reparation. *Suppuration*, or the formation of pus, is one of the common results of inflammation; but the different opinions which have been entertained respecting the nature of this process, arise very much from the different circumstances under which pus is formed. The author is of opinion, however, that some degree of inflammation is always necessary to the formation of pus; but that, when all irritation is removed from an ulcer, seton, issue, or abscess, the formation of pus will be diminished, or will cease altogether; while, on the contrary, inordinate inflammation will either change the qualities of pus, or altogether suppress its secretion.

Pus may be formed on the inflamed surface of the skin, eye, and mucous membrane, without change of organization; on the naturally unexposed surfaces of the body, when, in addition to an increase of red vessels, a new organization, by the deposition of coagulable lymph, usually, if not always, takes place; and on the surface of granulations. He gives a general account of the circumstances which vary the nature of pus,—whether these may be, the admixture of blood or serum; the extent of inflammation; the contact of bone; the operation of some specific disease, as gonorrhœa or cancer; or feebleness of constitution, as occurs in scrofula. He traces the interesting restorative processes which occur in the formation, and during the progress of an abscess; and is of opinion, that "the natural process, either for the removal of pus or extraneous matters, exhibits, in a striking manner, those elective actions of parts, which," says he, "I would ascribe to a local or organic instinct, and which, like the instincts of animals, produces the same results as intelligence."

Pus the author never considers as being in any way beneficial; what is called a kindly suppuration being still "an evil," though "in a mitigated form."

The *disorganization of parts* is one of the worst consequences of inflammation, and is most apt to take place in the fibrous, cartilaginous, and osseous structures, or in parts which have been regenerated, or formed in place of others, as cicatrices. The terms slough, gangrene, sphacelus, and mortification, have, the author thinks, been not always used with precision; and he is disposed to regard disorganization as of two kinds,—one which consists of an "unravelling of the structure of parts," from an effusion of serum, unrestrained by that deposition of coagulable lymph which limits the destructive effects of inflammation to a small space, and may be called *moist* gangrene; the other, which is not only attended with the death of the part, but with a new and peculiar arrangement of the animal substance, very different from what occurs when death takes place from mechanical injury. This consists in the conversion of the various substances involved in it, into a texture like wet tow, which never undergoes the putrefactive fermentation, but on drying, appears like brown or black wood. The author found it, after maceration had removed whatever was diffusible in water, to possess a cellular arrangement, "surpassing in fineness and delicacy any of the membranes of the body, if we except alone that that pervades the substance of the brain."

The next chapter is on the ***reputed consequences of inflammation.***

Though the effusion of coagulable lymph, and that species of absorption which is designated by the term ulceration, are usually, with Hunter, denominated *adhesive* and *ulcerative* inflammations, the author is of opinion that these processes are improperly so called; and that Hunter himself in his detailed accounts, distinguishes them from the real effects of inflammation. "It is well known," says the author, "that coagulable lymph may be thrown out by a natural and healthy action, as in the formation of the *decidua uteri:* that it is eminently conservative, in arresting hemorrhage from opened vessels; in the union of all the soft parts when divided; in forming the medium of conjunction of fractured bones, and in constructing the walls of an abscess, and of an aneurismal sac. Immediately on the receipt of an injury, also, lymph is shed before there is time for inflammation to set in. The surface of a wound that does not bleed is covered by a layer of lymph, in the very moment that the injury is inflicted. The inflammation which would ensue from the opening of a serous cavity is sometimes altogether averted, and almost always restrained within certain bounds by the effusion of lymph, uniting the opposed surfaces with each other."—Pp. 37, 38.

The circumstances under which the cure of hydrocele is effected, are not regarded by the author as affecting his position; for when injection is used, "the inflammation of the *tunica vaginalis* is prevented by the effusion of lymph," only such an injury being committed upon it, as "will furnish the motive for the effusion and organization of lymph, by which inflammation is anticipated and prevented." When, on the other hand, incision, caustic, or the seton are employed, inflammation and suppuration supervene, and after they have ceased, "the remains of the granulations serve as the uniting medium, as in any common wound." "As the arteries deposit coagulable lymph for the purpose of uniting parts that are in a fit state to be conjoined, the absorbents are endowed with the organic instinct, to remove parts, of which the organization has been so much impaired, either by internal action, or external influence, that they are not fit to remain as component parts of the body." This process of absorption is called *ulcerative absorption* and *ulceration*, and in pursuing the subject the author gives us the following lucid and interesting statement of the mode in which the absorbent and arterial vessels work together to one common end. "There are but few instances," says he, "either of natural or diseased action, in which the result is not conjointly effected by these two sets of vessels. In constructing the frame of the body, the absorbing vessels collect the materials, and remove the used and useless substances. In giving the proper shape to the different parts, the absorbents are even more important agents than the arteries. It would avail nothing to have new materials accumulated and deposited, unless the old were removed in the due proportion to effect the ultimate object.

"In all cases where parts are to be repaired or re-organized, the mutual operations of the arteries and absorbents are strikingly exhibited. When a bone is to be repaired after fracture, the arteries furnish the uniting medium, while the absorbents remove the projecting parts, which would interfere with the natural form. After amputation, at the same time that the wound of the soft parts is healing, the shape of the bone is modelled by absorption, to accommodate it to the ultimate figure of the stump. In cases of necrosis, the whole of the old bone, provided it be not exposed, is carried off by the absorbents, into the system, while the new bone is generated by the arteries of the vascular structure that invests it; and the regenerated bone, which is at first round, thick, and clumsy, is converted, in the end, by the absorbents, into a more perfect semblance of the original one, both as regards its shape and structure. In the cure of an abscess, as already mentioned, the organization of its cavity, and the progression of the pus to the surface of the body, are accomplished by the conjoint operations of the arteries and absorbents.

"When extraneous substances are to be removed, or decayed or dead parts are to be detached from the living body, the absorbents display a curious elective power, effecting just what is necessary, and no more than is necessary, if there be no inflammation. Thus, as much material is absorbed, as will be sufficient to separate a ligature from an artery, or to dislodge a piece of cloth from a gunshot wound. The same tendency to select, is exhibited in a great number of natural processes. As examples, we may mention the imbibing of assimilated parts of the food; the absorption of bile and urine, only when these fluids cannot be ejected by their natural outlets; the progression of the ova from the back to the front of the ovary, and the absorption of the dense tunic of the ovary, when the ova are to be discharged; the removal of the fangs of the deciduous teeth, and the preparation for permitting the secondary teeth to come into their proper place. In all cases, the absorbents seem to act under the influence of their own organic instinct, by which, in general, their operations are as beneficial, as if they were directed by the most perfect intelligence. There is no essential difference between the most salutary actions of the absorbents, and the most destructive ulceration: in all cases, these vessels are governed by the same law, which is, to remove parts that are not necessary, or not fit to remain. There is every reason to believe, that ulceration always takes place, because the vitality or the organization of parts have been impaired by inflammation, weakness, pressure, or other external injuries. All new formed parts and morbid growths are peculiarly subject to ulceration, on the slightest provocation;"

because their vital powers are low, and their structure imperfect: hence, cancer, fungous, and medullary tumours are sometimes rapidly destroyed by phagedæna or by slough. The structure in which scrofulous deposit exists, is liable, on slight excitement, to set up the ulcerative process. The inferior parts of the body, as they have less vital power than the upper, are most frequently the seat of ulcers."—Pp. 40—42.

The term ulcer, however, the author does not consider as a correct one, since its operation is compound, being more reparative than destructive; and is the result of an opposed action, "as the granulative succeeds the ulcerative process."

Dr Macartney now goes on to the consideration of *the different modes of reparation;* and this, it will readily be seen, involves the developement of the most material of the doctrines which he supports.

" Re-union and re-organization," says he, " are effected in four different ways, which may be designated in the following manner:

" *First,* immediate union without any intervening substance such as blood or lymph.

" *Second,* the union by the medium of coagulable lymph, or a clot of blood.

" *Third,* reorganization without any medium of lymph or granulations, the cavity of the wound becoming obliterated by a natural process of growth.

" *Fourth,* the reparation by means of a new, vascular, and organized substance, called granulations.

" To the first of these modes of cure," continues the author, "I should wish to give the name of *immediate.* The second may be called the *mediate by lymph or blood.* The third, being compounded of different actions, I find a difficulty in distinguishing it by a single name. It might be denominated the *approximating* or the *modelling* process of reparation, or that by a *natural growth.* The fourth mode of union, should be termed *mediate by granulation.*"—P. 48.

The *first* three of those methods of restoration, the author considers to be incompatible with the presence of inflammation. As examples of *immediate* union, he instances incised wounds of various extent, the lips of which are brought together ; the blood, if any, squeezed out; and the blood-vessels and nerves thus brought into perfect contact. The incision made in blood-letting is an example of such union, though it has been adduced by one of the first authorities in surgery, (Sir Astley Cooper) as a complete proof of the necessity of inflammation for healing an incised part, and as showing, that even this little injury " would infallibly destroy life, if this salutary process did not prevent it."

In all these cases, union takes place, according to the author, in a few hours, without the slightest pain, or any of the other characteristic phenomena of inflammation ; while, at the same time, he is of opinion, that the occurrence of such phenomena would retard the healing of the part.

When union takes place in the *second mode*, by the medium of lymph or blood, the parts are glued together, and in a few days have acquired some vascularity; not, however, according to the mode assumed by Mr Hunter, of an entire formation of new vessels, but by an extension of the old ones from the adjoining surfaces. This latter fact, the author has had occasion satisfactorily to demonstrate, by means of injections. When more lymph has been thrown out than is necessary for the immediate purpose of repair, it is absorbed, and usually leaves only a very thin white line of cicatrix.

The *third mode* of reparation, that by the *modelling process*, has not, the author informs us, been described by surgeons, because the existence of such a mode of reparation was unknown. It resembles very much, he tells us, what occurs in the lower animals; and consists, in the first place, of that close approximation of parts which takes place after the pain arising from the injury ceases, (which soon happens,) and when there is no tumefaction. There is " no necessity for effusion of lymph," to keep the parts together; and there are no granulations, because there is no cavity to be filled up. " The surfaces of the wound, although they come into contact, do not unite by vessels shooting across; they are smooth, red, and moistened with a fluid, which is probably serum, and present the appearance of one of the natural mucous surfaces of the body. If any parts have been killed by the injury, they are separated, by simply as much interstitial absorption as is sufficient to set them free. The wound is finally healed by the same means which determine the shape of the natural parts of the body. It gradually diminishes in extent until it is obliterated; or it may be cicatrized before the surfaces are abolished, after which the same process of natural growth goes on, until no part of the original wound is left."—Pp. 53, 54.

During this " modelling process, there is no inflammation in the parts, and the patients are so entirely free from all uneasy sensations," that the author states his having known instances " of them being ignorant of the real site and extent of the injury, until they had examined the part with their hand, or saw it in a looking glass."

We are not, however, distinctly informed by the author, as to the mode in which this operation is effected; for though he says the part is healed " by the same means which determine the shape of the natural parts of the body," the analogy is not quite apparent, nor the mode in which the process alluded to is conducted, perfectly palpable.

The formation of *granulations*, which is the *fourth mode* by which reparation is effected, takes place for the purpose of filling up a space created by the process of ulceration, or by the lips of a wound having been kept apart by means of some degree of inflammation. It sometimes happens, however, that the spaces now mentioned are filled up by lymph, which is never converted into granulations; but "the necessity of the granulative process seems entirely to arise from the parts being in that degree of excitement, which is not enough to prevent reparation altogether, but to permit it to be effected by a highly vascular medium." In simple fracture, where there is very little inflammation, lymph is the medium of reparation; in compound, where there is often a good deal, we have granulations; and the same with regard to opened joints. In these last, if inflammation is prevented, healing takes place by the effusion of lymph; if it go on, the cavity is filled by granulations; but these granulations, in the beginning only, take the place of the natural structure. They are soon absorbed, and by this means the edges of a wound approximate, and no more of the new medium is exhibited, than if lymph had been the substance employed in effecting the union. Granulations are composed of a fine cellular membrane, into which blood-vessels proceed from the subjacent tissue; and their granular form seems to the author to arise "from the force of circulation being unequally distributed, or directed, from the small trunks underneath, on particular parts of the surface of the sore." Granulations are large in proportion to the activity of circulation; they are covered with a delicate pellicle, somewhat similar to the almost invisible integument of the mucous membrane; and though nerves have not been traced into them, yet no doubt can be entertained of their existence.

The next chapter is on the subject of *Cicatrization*. "This term is applied to the last stage of reparation, in which a wound or an ulcer assumes a covering resembling, in some degree, the skin or other adjacent surfaces."

The author states the various circumstances under which it occurs; and the differences which exist, between the newly organized substance of which it consists, and the original material which it is meant to supply. He adverts to the practice lately recommended by Mr Higginbottom, (which has not, however, been successful in his own hands,) of imitating the natural process of scabbing, by producing, with lunar caustic, a thick and adherent eschar, whether on recent wounds or suppurating surfaces. The cessation of inflammation, and the absorption of the granulations are the necessary preludes to the formation of a cicatrix, which usually takes place on the edges of the ulcer, though it may sometimes begin at other points, and sometimes over the whole surface at once.

The author now goes on to the consideration of *Reparation in different Tissues.*

The power of repairing tissues does not seem to him to be regulated by any particular law, though it prevails, in the greatest degree, in the tissues "which are of a simple structure, and which are not endowed with peculiar, or very superior vital properties."

Loss of substance in the *brain* and *spinal marrow* is not supplied by a similar tissue; but *nerves* are so far regenerated as to be capable of resuming and carrying on their functions. Wounds of nerves heal by the formation of a hard bulb or nodule, whether by means of lymph or granulations. No nervous filaments have yet been discovered in the interior of the bulb; but the author has seen such filaments radiating from the bulb, at the place of healing after amputation, and proceeding to the skin. A preparation of the stump of a finger which belonged to the author, and is now in the Cambridge collection, exhibits this appearance; and a similar one is also mentioned by Professor Müller, as being seen in the divided nerve of a frog.

No part of a *muscle* is ever supplied in the higher orders of animals; but the deficiency is partially filled up by imperfectly organized coagulable lymph. *Tendons* unite so as to perform their functions; and when immediate union does not take place, as in laceration from dislocation, the ends of the fibrous structure form a number of "tags or little bulbs," by the effusion of coagulable lymph; and these become, in time, hard and smooth. *Periosteum*, the author states to be the only fibrous structure which is perfectly regenerated. The *dura mater* is supplied by dense membrane. *Cartilage* covering the ends of bones does not appear to be reformed. A sort of spurious cartilage occurs in false joints; but in old people, the ends of bones assume the "density, hardness, and polished surface of china or white glass," and thus supply the place of their original covering.

Where *serous membranes* unite, or *cellular membrane* is thickened, the lymph, in both cases, is in time so far absorbed as to leave little actual change in either tissues. *Mucous membrane* is never regenerated, nor are the villi of the skin, when the basis remains. The elastic tube of *arteries*, and the proper tissue of veins, are not regenerated; but their places are supplied by condensed cellular substance. In *fractures*, the processes of secretion and absorption repair the injury, by preparing a proper nidus for the deposition of osseous matter, the absorbent vessels gradually moulding the stump to the most convenient form, by carrying off its sharp and projecting edges. With regard to *necrosis*, the author informs us, that his ideas have been mistaken as to this process, when it is stated in Mr Cooper's Surgical Dictionary, that he attributes to the periosteum, the prerogative of forming the new bone; whereas, in fact, it has been found by Mr Hunter, that new bone can be produced after the periosteum has been completely removed. Dr Macartney's opinion is, that a vascular substance, called by him *vascular investment*, is created, which resembles granulations in structure and office, and which grows upon the periosteum, or whatever tissue may be near the new bone.

These granulations perform the double operation of absorbing dead bone, and depositing, at first a softer, then a harder, and at last an osseous matter to supply its place. But if ulceration should occur before the completion of the process, the portion of sequestrum which is exposed is removed by the process of exfoliation, if it be not by an operation. Such of the epidermoid tissues as are intended to be removed, when either periodically thrown off, or worn down, as the cuticle, nails, hairs, horns, hoofs, feathers, &c. are reproduced by a natural process similar to that of their original growth.

The next Chapter is on the *constitutional or remote causes of Inflammation.*

Plethora, whether arterial or venous, is viewed by the author as rather increasing the susceptibility to inflammation, than as an actual cause of it. *Local determination of blood* is to be regarded also in the same light; and it is always accompanied with a diminished approximation of fluid in other parts of the body. *Fever*, from the peculiar disposition to local affection of particular organs with which it is occasionally attended, is not an uncommon cause of inflammation; and "some physicians," the author observes, "from observing real inflammation to be occasionally combined with fever, have fallen into the error of supposing all fever to be essentially a disease of an inflammatory nature; and have mistaken the usual state of congestion in the vessels of the brain after common typhous fever, for the genuine effects of inflammation of the cerebral organ."

But inflammations sometimes make their appearance during fever, without being naturally or necessarily associated with it. In this case, however, they seem to be produced "by the disturbed state of circulation acting on organs already disposed to disease."

Certain morbid states of constitution lead to inflammations of peculiar character, particularly scrofula, gout, rheumatism, and cancer; but, with the exception of the first mentioned disease, the indications of which are much connected with want of general power in the system, the nature of the other complaints, and their mode of producing inflammation, are very little understood.

Suppression of the natural secretions, or of discharges which have been rendered habitual, is also a cause of inflammation; but the mode of operation is not quite apparent. The author is of opinion, that it cannot arise from the disturbance of the balance between the quantity of the circulating fluid, and that of the discharge suspended, which is often exceedingly trifling; and thinks it probable, therefore, " that the interruption of the secreting function, in any one part of the arterial system, and especially on sentient surfaces, may, from the love of sympathy, become a direct motive to the arteries, to assume in certain places an inflammatory state." *Affections of the skin* afford some curious examples of this kind; and Dr Jenner, who had a red spot on his wrist, of the size of a sixpence, told the author that, whenever it faded, " his stomach and general feeling of health were affected."

Getting cold is one of the most frequent causes of inflammation; and of all parts of the body, the back is that which seems to the author to be most susceptible of cold " insidiously applied." Prostration of strength and dejection, or want of interest are very favourable to taking cold; but strong mental excitement or attention renders the body much less liable to the impression. Hence, soldiers during a march, persons engaged in field sports, or even in the repulsive occupation of exhumation, almost always escape danger from cold and wet.

The *local causes of inflammation* are stated by the author to consist in, *first*, " palpable injuries to the organization, which affect the structure in such a way, as to interfere with the performance of its functions; *second*, the application of substances which irritate the sentient extremities of the nerves; *third*, fluids, which produce a peculiar impression, and that create a disposition to specific inflammation; and, *fourth*, sudden changes in the state of feeling of parts, although, in some instances, to one more agreeable than that which previously existed."

The *first division* comprises the various descriptions of *wounds*, whether incised, lacerated, contused, gunshot, or punctured wounds. Of those the latter are the most severe; and the mischievous character depends very much on the parts being forced asunder, as by a wedge. Wounds produced by the teeth or claws of some quadrupeds, by the spurs of the cock, or the spines of some fish, are peculiarly severe. There is an idea, indeed, among fishermen, that the spine of the *sting ray* emits a poisonous fluid; but the author has carefully dissected that part of the fish, and is satisfied that it is quite devoid of any poisonous apparatus.

Various mineral substances, particularly mineral poisons, act powerfully in producing inflammation; as do high temperature, friction, the natural temperature after exposure to severe cold, and also concussion, pressure, constriction, and uneasy posture. Internal and external surfaces that are opposed to each other, are more disposed to sympathize, than tissues that are continuous;

and hence it is, that serous surfaces may become inflamed from sympathy with the inflamed skin, without the interjacent tissue being affected.

Certain animal fluids, but particularly the introduction of dead animal matter by a wound, produce serious inflammations of a peculiar character; but the author has found, that in his dissecting room at Dublin, no serious injury occurred during the space of fifteen years, from simply washing the wound with a solution of alum and water, and keeping it wet for a few hours with the same fluid. Putrescent animal fluids are the most feared; but there is really the greatest danger from the introduction into the system, of recently dead human animal matter, particularly that of the brain, and of effusions into the abdominal cavity, more especially after puerperal peritonitis.

Fluids of recently killed animals are never found by butchers to produce irritation of skin; though when putrescent, they do so; but cleaning of tripes, paunching hares, and horse killing, sometimes produce erysipelatous inflammation on the hands of such as are engaged in those occupations.

All these local causes of inflammation operate, as the author thinks, by making "such impressions on the sensibility of parts, as dispose the arteries to assume an inflammatory state;" and hence a considerable extent of mechanical destruction, as blowing off the hand or foot by gunpowder, may excite less inflammation, than a small punctured wound among the tendons of the hand or foot, where organic sensibility is so much concerned.

That impressions on the sensibility are the sole causes of inflammation, by the production of a "new state of sensation" in parts, the author considers as proved by the occurrence of inflammation on opening a cavity. In this case he does not regard it as depending on the introduction of air; for injection of air into the cellular membrane has no such effect, nor has its introduction into the cavity of the chest, by an ulcer of the lungs opening into the pleura, nor a mere opening into the abdomen, which, from the viscera filling the cavity completely, does not admit air into its interior. "Organic consciousness or sense of exposure, or rather of imperfection," gives rise to inflammation, as the author thinks, by taking away support, as in tapping hastily for dropsy; in parturition, or the operation of lithotomy. "The sudden removal of the stone from the bladder by lithotomy, although there is a transition from pain to ease, becomes a motive to inflammation of that organ, and of the peritoneum; and in such cases the bladder is frequently found flaccid, thereby creating the organic consciousness of vacuity. For the bladder in a natural state always adapts its size to its contents, and consequently is at all times equally full."

We now arrive at the author's section on *the Proximate Causes of Inflammation.*

The elastic or middle coat of the artery altogether disappears in its fine branches; and the simple membranous tube which constitutes the remainder of the vessel, besides possessing "very considerable elasticity," is endowed, as the author thinks, with other "most extraordinary properties." While the operation of the elastic coat is to aid the heart in the transmission of the blood, the membranous tube, which constitutes the smaller division, acts very differently. It possesses, as he thinks, with other tissues similarly constituted, "a positive and active extension and dilatation," since dilatation can take place where it "could not be produced by a mere cessation of the contractile action. When vessels are dilated, or surfaces extended without mechanic force, the causes," he considers to be, "some excitement or irritation, or a necessity for the performance of some new or more vigorous functions;" and hence he thinks we have a right to assume "that the dilatation or extension of parts is their most active condition, and their contraction the negative or passive state." This active condition he terms "*tone or tonic action*," which is possessed in common by arteries, excretory ducts, and the skin itself. It lodges in the "white reticulated structure, which constitutes the basis of the skin," and in arteries is principally confined to the inner coat. In inflammation the arteries dilate, and consequently receive more blood than they ordinarily contain, while the colourless vessels admit coloured fluid, and the coats of those which ordinarily convey red blood increase in size and strength. Dilatation is, therefore, "the result of some excitement, and is attended," as has been just mentioned, "with increased vascular power and energy of function; while, on the other hand, the contraction of arteries is the consequence of diminished vital power."

This, therefore, is the author's theory of inflammation, which has a considerable resemblance to that of De Gorter; and he employs, as illustrative of his argument, the power of extension possessed by various excretory tubes,—as the biliary ducts and the urethra, in the passing of a calculus; by the integuments of the perinæum during parturition; and by those of the anus of birds in the passing of an egg. All these phenomena are considered by the author as "vital and positive acts." He considers the terms "increased action," and "increased contraction," to be applied to arteries without definite meaning; and thinks we need not seek for new modes of accounting for the phenomena of inflammation, "if we acknowledge that the small arteries have the power of assuming a greater or less capacity."

Small arteries are so intimately mixed with contiguous tissues, as to make it difficult, if not impossible, to ascertain their degree of sensibility; but they seem to possess an extraordinary sympathy with various structures, both near and distant; and the author thinks, " that, as we have sufficient grounds for assuming that an organic sense of injury or danger, modified according to the causes which produce it, necessarily precedes inflammation, it is reasonable to conclude that this is the *proximate* or *essential* cause of inflammation." This organic sensibility being derived principally from the sympathetic system of the nerves, may exist after ordinary sensibility is done away, either by paralysis or division of nerves. An example of this kind is afforded in the horse's hoof, which, after the division of the nerve that supplies it, is equally liable to inflammation as before; but, at the same time, the author is of opinion, that if it were possible to deprive an artery of its own peculiar nerve, then none of the phenomena of inflammation could be produced, " whatever injury might be inflicted."

The author does not consider the experiments made by the application of various stimuli to arteries, as, for example, those of the frog's foot, as fair ones, because the operation, at least in the first instance, is on the skin of the animal. When acids produced contraction on arteries, they would have done so on " them if dead;" and electricity and galvanism have no power of making an artery contract. The author laid bare the carotid artery of a dog; and after including a part of it between two ligatures, to take off the force of circulation, he made a hole in the vessel with a pin, and applied galvanism without any effect; but on slitting the vessel up, and removing the blood, the vessel contracted to two-thirds of its previous circumference, " showing very strikingly the disposition which the arteries, in common with the other tonic tissues, have to collapse, on the motive or necessity for their existing in a dilated state being removed."

In the next chapter, which treats of *the species of Inflammation*, the author points out the most important difference between phlegmonous and erysipelatous inflammation, and considers catarrhal inflammation, as well as that which accompanies certain vesicular diseases of the skin, as having a good deal of the latter character.

In a chapter on *Congestion, as contradistinguished from Inflammation*, the author notices the pathological errors so frequently made in examining dead bodies, of mistaking congested for inflamed parts; and particularly adverts to the necessity of accurate discrimination in relation to the stomach, where he has often seen those appearances " set down as evidences of inflammation, which would be an awful mistake to make, in cases where there was any suspicion of poison being administered."

In *congested parts*, the author states, what he informs us has not before been described, that arteries are found to be smaller than their natural " size." In an experiment of tying both jugular veins of a rabbit, the animal died apoplectic; the veins lying on the outside of the ear being much enlarged, while the corresponding artery was reduced very much below its natural size. The author does not mention, however, the state of the artery while life continued, though in an experiment made, in which the trunks of several mesenteric veins of a young rabbit were tied, "the corresponding arteries contracted immediately in the most palpable manner, and to a very small size."

Transudation of blood after death, through the coats of veins, into the surrounding cellular membranes, is a common and well known occurrence; but the author states, on the authority of Mascagni, what has often been observed by himself, that the blood is frequently so fluid, in many cases of congestion, as to infiltrate through the coats of veins into the cellular texture during life.

We now arrive at the last chapter of Dr Macartney's work,—that in which he treats of *the remedies for inflammation*. These he divides into the following seven heads, namely,

"1st. Remedies which diminish the force of the heart, and give the disposition generally, to the small arteries to go into the contracted state.

"2d. Means that effect a diminished size of the arteries, or reduce the sensibility in the inflamed part.

"3d. Medicines that augment or reproduce the natural secretions, and thereby abate the circulation, or lessen the effusions made into inflamed parts.

"4th. Counter-irritations, secretions, or impressions made in different parts from those which are inflamed parts.

"5th. Lotions or fluids which exert sedative and astringent power.

"6th. Means for affecting in an agreeable manner, the sensations of inflamed parts.

"7th. Causes which produce an easy or satisfied state of feeling, on the sentient surfaces, or in the individual."—P. 143.

In the *first head* of curative means, he mentions the employment of *nauseating doses of emetic tartar*, as, for example, a table-spoonful, every half hour, of a solution of one grain in a pint of water; but the most important agent is the use of *general or local blood-letting*. The author gives some good general directions as to the best modes of performing both venesection and arteriotomy; and thinks, with much truth, that sufficient attention is often not given, and skill exercised, in the performance of those very important operations. He notices the different views which should guide the practitioner, in the extent and urgency with which the remedy is to be employed, according to the nature, texture, and functions of the part affected; and is of opinion, that there is a great error in the common practice, of drawing blood the moment an injury is received, long before there is time for inflammation to set in; and that this plan is worse than useless, by depriving the practitioner of the means of acting with sufficient energy, when the proper time arrives. The author thinks that, in severe external injuries, general bleeding may be dispensed with, provided the other general principles which he has laid down are judiciously followed.

In *topical bleeding* blood is usually taken from vessels in the neighbourhood of the inflamed part, rather than from the inflamed vessels themselves; and the object, Dr Macartney thinks, is to make "such an impression on the sensibility of the part as will produce a contracted or diminished state of the arteries, by which less blood is permitted to circulate in the vessels immediately concerned." This disposition is described as consisting in an "organic consciousness of vacuity, which always produces a tendency to contraction."

Leeches and *scarification* act differently, according to th author; for by the slow and insidious mode in which the "bloo steals away" with leeches, "a greater sense of danger is felt i the arteries, and consequently a greater disposition to contrac than by a large hemorrhage, if effected by different means."

Abstraction of heat, by the operation of a moderate degree o cold, is one of the most effectual of the *second division* of cu rative means; but the usual mode of applying refrigerant lotion is an imperfect one; and the method of *irrigation*, as it is callec which is used by the French, consisting of an apparatus for drop ping a cold fluid on a limb from above, though an improvemen is clumsy and inconvenient. The author places the limb of patient in a trough, covers it with lint, and then conducts th water from a contiguous vessel, by means of a broad stripe o woollen cloth, acting as a syphon, which is to be inserted in th vessel of water, and is to terminate in a point when it touches th lint.

Professor Weidlick has employed a double catheter for con veying a fluid application to the bladder; the patient sitting o a chair, to the back of which is adapted a reservoir, communicat ing by a tube, with a stop-cock to it, which is fixed to the cathe ter, and conveyed under the seat. The author has used simila means for the rectum, vagina, &c.

Intense cold is also a remedy in severe inflammation; but car must be taken that it is not withdrawn too suddenly. Ice or iced water are better than frigorific mixtures; but cold in a moderat degree by means of cold water, is generally a useful applicatio when the inflammation is not very violent, and is accompanied b heat. In the application of cold, however, the feelings of the pa tient, more particularly in respect to its power of alleviating pain are the best tests of its efficiency.

Among the *third class* of curative agents in inflammation ar mercury, diaphoretic medicines, and the warm bath; but mercury is considered by the author as the most important, and he views it as more particularly beneficial in inflammations of the air-passages, of the pericardium, of the serous cavities generally, of the glandular structure, and of the internal parts of the eye.

We must be allowed, however, to doubt the extent of Dr Macartney's experience in relation to fever, when he states, that the beneficial operation of mercury "is exhibited in the most striking manner in that disease," when uncombined with visceral affection, or unusual debility; and "that in *no single instance*" of such affection, had he known it "fail in arresting the progress of the disease."

The *fourth head* of curative means, comprizes rubefacients, blisters, the various modes of producing pustules and pustular discharges, burning with moxa, and the formation of issues and setons. The operation of counter-irritants is very obscure; but the author deems it essential to their favourable operation, that the space placed between the parts to which they are applied, and those affected, should be entirely free from inflammation, although it may be very small, and the two inflammations consequently very near each other. In erysipelas, blisters, where they are serviceable, do good by producing an inflammation of a more defined form, and more curable character than the original disease. The deep incisions which have been recommended by Mr Lawrence, and Mr Copland Hutcheson, the author is of opinion, are to be viewed in the same way as moderating the malady; but he does not think such extensive injuries are at all necessary, or that "the cases recorded" appeared "to justify the practice."

Of the different *medicated lotions*, which form the *fifth division* of curative agents, those containing lead are the most effectual; and the author states, though we fear with rather too much confidence, that the lead lotion never fails to cure the most obstinate *tinea capitis* when properly used. He advises to cut the hair close, apply poultices to bring off the crusts, and then employ the lead lotion frequently with lint, covering the head with Indian rubber or oil skin, to prevent evaporation.

The best means of producing *agreeable sensations* in inflamed parts forms *the sixth class of remedies.* Of these, the application of steam is one of the most efficient; and the author considers this as arising, in an important degree, from the intimate interweavement of the nerves with the blood-vessels of the skin, and from the former consisting of a junction of fibrils from both systems. *Fomentations* are very serviceable; but they do much less good than the uninterrupted use of steam; and the author, from a conviction of this circumstance, invented in 1820, an apparatus by which steam could be very conveniently applied. It is sold by Stoddart of the Strand, and consists of a lamp and small boiler, into which not exceeding a pint of boiling water is to be put, with a funnel-shaped top, which is to be adapted to a tube of woollen cloth about twelve inches wide and three feet long, held open by circular pieces of whalebone. This tube may be adapted to any part of the body to which the vapour is meant to be applied; and by varying the mode of application, the whole body may, if necessary, be enveloped; while by including the mouth and nose in the end of the tube, the apparatus may readily be made subservient to inhalation.

When the remedy is used immediately after "lacerated, gun-shot, and punctured wounds, contusions of bones, fractures near joints, recent luxations, bruises and strains of joints, and in all wounds accompanied with a peculiar overcoming pain, and a shock to the nervous system, it removes all pain and consciousness of injury in a very short time."

After the pain and sense of injury have passed away, steam, at a lower temperature, may be advantageously continued.

The application of water, (to which the author applies the term *water dressing*) he recommends as exceedingly useful, from the temperature of the body to the freezing point; but in general it is to be used of the heat of the body. Folds of soft lint, wet just so much with warm water as not to drop, and renewed four or five times in the twenty-four hours, are to be applied to the part, which is to be covered with oiled silk or Indian rubber. He considers this application as greatly better than poulticing; and views it as having a tendency to promote the cure of wounds and ulcers not requiring excitement, "by the approximating or modelling process" already mentioned. In gun-shot wounds, the practice has been found very advantageous; and when it is employed, there is little disposition to the occurrence of tetanus. The author gives an interesting history of the application of water to wounds, from the earliest periods; and mentions the admirable effects observed by the Barons Percy and Larrey, and Professor Kern of Vienna, from its copious employment, adapting the temperature, of course, to the season, or the circumstances of the case. The author, therefore, does not claim the merit of a discoverer of this remedy; but, says he, "that I have been the means of introducing it to the attention of the profession in these countries, is a matter of too much notoriety to admit of dispute. I have also connected the use of it," he adds, "with general views of the nature of inflammation, which (whether right or wrong) are peculiarly and distinctly my own; and I have demonstrated the possibility of open wounds healing without inflammation, and without the medium of either coagulable lymph, or granulations; a fact which, as far as my information extends, has not even been hinted at, by any writer on surgery, either ancient or modern."

Circumstances which produce *agreeable feelings*, either on sentient surfaces, or in the individual affected, are important adjuncts in the cure of inflammatory diseases, and form the *seventh* and last class of remedial agents. Freedom from restraint, as to position, from pressure, as by bandages, (for in few cases only these are absolutely necessary,) and from friction and motion are much to be inculcated. Elevation of the affected part is advantageous; as is likewise attention to every means of taking away irritation from the sentient extremities of the nerves, either of the skin by means of baths; or of internal surfaces by the use of mild purgatives. Pure air, and cheerful impressions are eminently useful. Continual exposure of wounded surfaces to hot and dry air was found by Dr Jules Guyot to succeed well with rabbits, but not in the human subject, from the difficulty of having the inflamed limb confined so long as is necessary, in the box which he employed for the purpose; but Dr Macartney thinks the plan deserves a trial, if it could be made by using a flexible material in which to confine the limb; and if the water-dressing were employed previous to the warm air.

We have been induced to communicate in considerable detail, the most material facts and reasonings contained in the work before us; as well from their importance, as the general interest of the subject, and the estimation in which we hold the character and talents of the author.

The success which attends the diminution of inflammatory action in promoting recovery in a great variety of injuries and ailments, is strikingly evinced by many of the statements given in the present treatise; and much advantage will, we have no doubt, be derived, both with a view to prevent, and to cure inflammation, by the use of many of the modes which the author recommends as the lenient method of treatment.

His opinions in respect to the peculiar process which is adopted by nature in the cure of wounds, very much resemble those which were brought forward, nearly forty years ago, by Mr John Bell, in his Principles of Surgery. That acute and able surgeon, in treating of adhesion, has the following remarks: "The adhesive inflammation (for it is called inflammation) is not attended with fever, pain, swelling, nor redness, unless in the most trivial degree; that gentle swelling which indicates the fulness, and strong, but healthy action of the vessels, an adhering part must have; but this increased action of the vessels during the reunion of the lips of a wound, stands on the same footing with the healthy action of vessels in forming or in supporting any part of the system." "From the very first moment of adhesion, the vessels of a wounded part resume their healthy action, unaccompanied with inflammation, swelling, or pain; the part is once more entire and sound. If the vessels become thus entire from the very moment of their reunion, if neither pain nor swelling come on, unless the process fail, how can this be called a disease? Or, by what sophistry can it be comprehended under the definition of an inflamed part? To speak thus, appears to me an incorrect and unfavourable view of the matter. It is to describe the cure by the very name of the only disease which can interrupt the cure." "Adhesion prevents inflammation: when the parts adhere, they enter into a healthy action; they are entire, and they do not inflame."

According to the views entertained, both by Mr John Bell and Dr Macartney, (in which our readers will, from the extract just given, see a remarkable coincidence,) the term adhesive inflammation is considered as inapplicable to the phenomenon of union by the first intention. Mr Bell is of opinion, that "each cut surface throws out a mucus," as the medium of union, which become vascular; but Dr Macartney, in his *first* and *third* modes of reparation, represents the process of union as being effected without any medium whatever. In the *first*, he considers a vascular union as taking place; "the divided blood-vessels and nerves" being "brought into perfect contact;" but whether vessel unites with vessel, and nerve with nerve, or in what other way junction may be supposed to be occasioned, we are not informed. In the *third* mode of reunion, it is merely stated, that divided parts which are disposed to spontaneous contact, remain together, "but do not unite by vessels shooting across;" and that the wound gradually diminishes, and the part heals by what he terms "the modelling process," a means which he considers as similar to those "which determine the shape of the natural parts of the body." We have already stated that the method by which this operation is represented as taking place is not very apparent; and we must be allowed to remark, that there seems to be no reason for a difference between the process of healing when the parts are kept in contact by easy and unirritating means, and where, by the spontaneous approximation of parts, no artificial modes of preserving contact are necessary. When raw surfaces are in contact, and uninflamed, they form an immediate union by the first mode of reparation mentioned by the author; but the author does not point out any circumstances which can prevent union taking place, in precisely a similar way, if contact is equally close and constant. It would appear, indeed, according to his view of the subject, that the parts may heal externally, and yet the surfaces below not be united, or, as he states, not be "abolished;" and that, therefore, till the "process of natural growth goes on" to such an extent as to leave "no part of the original wound," there must always be a greater liability to injurious impressions, than if the whole part were perfectly sound, and in a complete state of reparation. He admits that in the "modelling" process, the raw surfaces are moistened by a fluid, which he thinks is probably "serum;" but, why there should not ooze, from such surfaces, when once the flow of blood has ceased, and from the lips of wounds which he describes as uniting by the *first* mode of reparation mentioned by him, a secretion of lymph, such as occurs in the *second*,—appears to us to be contrary to all pathological analogy. Even in the ready healing which takes place in the lower classes of animals, the author states that there is some sort of secretion thrown out from divided vessels, by which nature effects, or which at any rate accompanies, a cure; and there seems to be fair evidence for concluding, that a similar curative operation is adopted in the higher classes, by means of the vascular secretion to which the divided extremities of vessels in the first instance give rise.

It is undoubtedly much more philosophical to leave a phenomenon totally unaccounted for, than to be contented with vague and uncertain evidence in relation to it; but yet, as the author admits the intermedium of lymph in his *second* mode of reparation, when the parts do not admit of perfect contact, but when, at the same time, there is no inflammatory action, it is difficult to conceive what should prevent the throwing out of a similar secretion when the parts approximate. It seems to be unquestionable, that, as soon as the hemorrhage from a recent incised wound stops, lymph is thrown out from the divided vessels; and that this lymph agglutinates the parts divided, and in time allows a free circulation across the united parts, though it is not yet precisely known in what way the vascularity or vascular union necessary for the purpose is effected. Taking any of the examples adduced by the author, of slight incised wounds, it will be found on accurate examination, that very soon after infliction, and before the occurrence of any phenomenon which can strictly be said to belong to inflammation, a partial adhesion or agglutination takes place, which will soon become perfect reparation.

Much of the difficulty which has occurred in respect to the phraseology connected with inflammation seems to have arisen from the great variety of circumstances under which coagulable lymph is thrown out. Sometimes it is a mere medium of union, and, in this case, passes from vessels which differ little or nothing from their ordinary state, but in being divided; and sometimes it arises from a part which is actively inflamed, and, in this case, when it attacks serous membranes, is almost always attended with adhesion of one surface to another. The power of producing adhesion, being, therefore, so common a result of inflammatory action, it has given rise to the application of the term adhesive inflammation, rather more extensively than is quite consistent with philosophical precision. But it does not appear to us that the error in this respect is to be attributed to Mr Hunter, or that Dr Macartney, we may be allowed to say, in stating that Mr Hunter "seemed aware of the possibility of wounds healing without inflammatory action," apportions quite the degree of credit which is due to him for discrimination and accuracy. The notice taken of Mr Hunter's observation on this point is so slight, as to give it the semblance of being regarded by the author as an accessory remark; and the fact itself, as an apparent exception only, to the principle of adhesive inflammation, which he was desirous of establishing. Nor does he seem to have entered entirely into Mr Hunter's views on the subject, in representing him as entertaining a belief of the necessity of the process of inflammation to the healing of any wound; and as having employed the term *adhesive inflammation* to indicate the process adopted by nature for such reparation; a term which the author regards as "unhappily," because in some degree, inconsistently and erroneously used.

It may, however, be fairly inferred from Mr Hunter's work, that he viewed union by the first intention as a totally separate and distinct process from that of inflammation; and he, therefore, devoted the first chapter of the second part of his treatise (amounting to above 30 pages) to the consideration of this subject alone; while, in his second chapter, he goes on to the developement of his views in relation to the fundamental principles of inflammation, by which he is led, in the third chapter, to treat of adhesive inflammation. It would lead us into too much further detail, to advert to every important point in Mr Hunter's work which relates to this subject; and we must, therefore, satisfy ourselves by noticing a few of his remarks, which seem to be decisive of an interpretation of his views.

"When wounds are made in weak habits," says Mr Hunter, "there is a great backwardness in the two cut surfaces to unite by the first intention, and, therefore, *inflammation* takes place if there be strength of constitution to produce it." * (P. 231; 280 new edit.) "Deficiency in the power to heal becomes a stimulus, or an incitement to *inflammation*." (P. 235; 283 new edit.) "Wounds of the scalp have a firm union in twenty-four hours." (P. 200; 249 new edit.;) but in case of the occurrence of ecchymosis, "something similar to the *adhesive inflammation*" takes place, "to set bounds to the extent of the bag." (P. 198; 247 new edit.) If "the salutary processes go on readily, no other effect of injury, or irritation, or pain, in consequence of nature's operations is felt. No universal sympathy or fever takes place, except what arises from the mere injury done; but all is as quiet as if nothing had happened. * * * But in some circumstances, the accident becomes the cause of irritation, and produces another operation of the parts *called inflammation.*" (P. 201; 249 new edit.) If violence is so great, or restoration not so ready, as to "exclude irritation," we have an action "taking place in the parts *called inflammation.*" (P. 248; 295 new edit.) If the divided parts are not brought immediately together, "*inflammation* will inevitably follow;" and "this inflammation I have called *the adhesive.*" (P. 206; 254 new edit.) "Nor is it even necessary that *either* surfaces should be in a state of inflammation to admit of union." (P. 252; 299 new edit.) "*Inflammation*" (after accidents) "does not begin for twelve, eighteen, or twenty-four hours." (P. 304; p. 347 new edit.) "It," (the *adhesive inflammation,*) "is often of service in those cases which arise from violence, although not so necessarily so, the injured parts not being always under the necessity of having recourse to it, as was shown in treating of union by the first intention." (P. 364; 399 new edit.) "The *adhesive inflammation* takes place in consequence of accidents, when it is impossible it should even produce the same good effects, such as in wounds, which are not allowed, or cannot heal by the first intention." (P. 368; 402 new edit.)

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* A Treatise on the Blood, Inflammation, and Gun-shot Wounds. By John Hunter; or the Works of John Hunter, edited by J. F. Palmer, Vol. iii. London, 1837.

These extracts will, we think, justify our inference, that John Hunter considered union by the first intention and adhesive inflammation as different processes; and that, whatever improvements a longer life might have enabled him to make in his Treatise on Inflammation, he would not have been "compelled," (as the author thinks would have been the case,) "to deny the necessity of the process of inflammation to the healing of any wound," for this simple reason, that he had never maintained that opinion.

When the able surgeon whose opinion the author quotes, as decisive of the sentiments at present entertained in respect to the identity of the process of union by the first intention and adhesive inflammation, it is pretty obvious that he applies his observation to the effusion of coagulable lymph; and that thus, if it did not take place in an opened vein, from a similar cause to that mentioned by Mr Hunter, as sometimes keeping a wound in the abdomen open after tapping, the consequences would be serious.

How far union by the first intention may be completed, even in the slight puncture which is made in blood-letting without the occurrence of some degree of tenderness, indicative of slight inflammation, may perhaps admit of some degree of doubt; but that the effusion of coagulable lymph, so necessary to the union of the lips of a divided wound, can take place without, or, at all events, previous to the occurrence of any of the phenomena of inflammation, and, therefore, it may be presumed, independent of such occurrence, seems to be by no means an unfair conclusion. A constant product of adhesive inflammation, it appears to us, is necessary to the healing of a wound by the first intention; but a similar product takes place from divided vessels previous to inflammation coming on at all. This is not altogether the author's opinion; but he deserves much credit for calling the public attention to the facility with which union may take place in divided parts, and to the importance of preventing or keeping down inflammation in them, as not only not necessary to such union, but as sometimes having a tendency to retard it. In the views which he has presented on this and the other subjects of which he treats, and in the practice founded upon them, he has given an important impulse to modern surgery, and in particular to the doctrines of Hunter, which he has followed out in a lucid and satisfactory manner.

We have not always agreed in the correctness of Dr Macartney's conclusions; and if the reasoning which he employs in the more speculative parts of his subjects has sometimes appeared to us rather hypothetical, we have always thought it ingenious and interesting.

Art. II.—*Traité Pratique des Maladies Veneriennes, ou Recherches Critiques et Experimentales, sur l'Inoculation, appliquée à l'etude de ces Maladies.* Par Ph. Ricord, M. D. Pp. 808, 8vo. Paris, 1838. *A Practical Treatise on Venereal Disorders, or Critical and Experimental Researches on Inoculation, applied to the study of these disorders.* By M. Ricord.

In every disease whose presence is indicated by local evidences, it is interesting and no less important to inquire, whether these appearances constitute the disease, or are mere indications of a constitutional malady. The subject is interesting, because its tendency is to excite a perception of the beautiful constitution of the animal economy; and it is important, because it is by detailed inquiry of this kind that the great principles of the science are established.

The strictly local nature of primary syphilis is no longer a question. It is a demonstrated truth. It is equally certain, however speculatists may doubt, that, from the local infection, results a constitutional disease, which is at once dangerous to the individual and his posterity. As the hope of eradicating syphilis is about equal to that of chasing other contagious diseases from among mankind, it becomes an object of prime importance, to learn how to mitigate the severities of a plague. Early recognition of disease is the first great step towards lessening its evils; and if this be true as regards disease in general, it is especially so, as concerns chancre in particular.

Inoculation has often been applied to the diagnosis of venereal disorders; but the unerring accuracy of the test has only been fully appreciated of late years. In the work before us, this subject is treated in detail, and with a precision which is truly admirable, and deserves the attention of the profession.

Like the majority of writers on the venereal disease, M. Ricord commences with proving the materiality of the syphilitic virus. But this seems to be a work of supererogation; for although the profession has been frequently surprised, and sometimes puzzled by the fanciful reasonings of hypothetical writers, no real argument exists against the entity of the poison of chancre. We may safely defy, alike the most subtile and profound, to prove the negative proposition, that regular unvarying phenomena, the constant successors of certain antecedents, are not what they are seen to be.

That peculiarity of constitution modifies the appearance and progress of local disease, is a position maintained by every scientific physician; but it seems to have been forgotten that chancre is amenable to the same law; and out of this forgetfulness has issued a cloud of hypotheses which shrouded what was previously sufficiently dark. Difference in form is the ground-work of the argument in favour of a plurality of syphilitic poisons; for it has been imagined that, because the matter of chancre is specific, the effects of the specific cause should always be identical in appearance. This no doubt would be the case, if all constitutions were alike, and the same tissue in each the seat of disease; but constitutions vary, and each tissue is endowed with a peculiar irritability. Hence, when the same specific irritant is applied, the effects, though identical in nature, will be different in form. Thus the multitude of forms which chancre may present forbid that its diagnosis should be founded on its external characters alone; and while inoculation is valuable as a differential means of diagnosis, it becomes invaluable as a method of certifying the specific nature of sores which, to appearance, are not chancres.

It has been remarked, however, that inoculation is not a test for syphilis; that there is no characteristic syphilitic pustule; and that, excepting in the case of Hunterian chancre, the disease can only be certainly known by its effects. Now if the inoculation of the secretion of a sore, known by its external characters to be syphilitic, viz. the Hunterian chancre,—give rise to a pustule of definite character in every case; if, as the disease goes on, this pustule becomes a sore which secretes a matter possessing the same properties as that from which it originated; we have a right to conclude, not only that the disease thus artificially produced is chancre, but that all sores secreting matter possessing the same property are chancres likewise. If our opponents are in the right, then medicine can no longer be a science ; the doctrine of H must be true ; and disease may be only an idea existing in brain of the physician.

Inoculation has been condemned as a means only of satisf curiosity, while it multiplies the sources of danger ; and it has b argued that, for this reason, it is a practice which ought not t tolerated. But if truth be the object of our search; if succes treatment depends on a just diagnosis; if diagnosis be the basi prognosis; and, above all, if it be true, that the number or size of primary sores has little influence in determining the occurrenc constitutional symptoms ; this objection can no longer be vali the eyes of the rational physician. Empirics live on our ignora and the acquisition of truth is the surest way to rescue mank from the fangs of two ruthless destroyers,—the syphilitic poison quack-doctors.

Mankind is deeply interested in the solution of the questi *what is syphilis?* and although this scourge has too long b an object of observation to admit of difficulties in replying to general question, still the puzzling inquiry is daily made, is *t* sore a chancre? Such a problem can now readily be solved; M. Ricord has demonstrated that chancre in the period of incre or when stationary, is always inoculable, and gives origin to the c racteristic pustule alluded to above. Chancre, in order to be p perly understood, must be studied in its different phases. T disease consists of two stages or periods ; *1st*, the period of increa or the stationary period, in which it furnishes an inoculable sec tion ; and, *2d*, the period of reparation, when it assumes the fo of simple ulceration, and is no longer contagious. The impo ance of this distinction is too evident to require being insisted

If we inoculate the matter secreted by chancre during the fi period, constant and regular phenomena take place. In twent four hours the part becomes red as after vaccination, and passi through the stages of vesicle, pustule, and incrustation, it final assumes the form of the Hunterian chancre, and engenders a vir proved by experiment to be identical in nature with that whi produced it.*

Numerous experiments and observations authorize the follo ing conclusions.

1. Chancre can only be recognized with certainty, by the qu lity of the matter it secretes, and the constitutional symptoms determines.
2. Chancre alone can produce chancre.
3. Inoculation never fails, if the proper conditions are observ regarding the taking and applying of the matter.

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* The phenomena which occur on the inoculation of the matter are fully c scribed in a former paper ; *vide* Medical and Surgical Journal, No. cxxxv. A 1838.

4. The matter of the pustule of inoculation is equally virulent with that of the original sore.

5. The pustule is always developed on the precise point where the inoculation was performed, and never at another.

6. The chancre of inoculation is never preceded by phlegmon, unless the matter has been introduced into the cellular tissue or a lymphatic vessel.

7. The constitutional malady which results from this antecedent only, is not a *necessary* consequence of it, and appears only when the primary disease has endured for a certain time.

8. In order to perceive the truth of this important observation, it is necessary to distinguish between the real and factitious commencement of the disease, that is, to date its commencement from the day on which it was contracted, and not from that on which it was first perceived.

9. By making observations in this way, it will be found that, if the sore be destroyed with caustic or other means on the third, fourth, or fifth day after the application of the cause, all risk of constitutional infection is removed.

10. Indurated chancre is the common antecedent of constitutional syphilis; induration commonly commences on the fifth day, it apparently announces that the poison is entering the system, and, in so far as it has not occurred, the disease may be regarded as still local.

M. Ricord's experiments further prove,

1*st*, That the fact of an individual *having been* or *still being* the subject of chancre, does not prevent his contracting other chancres to an indefinite number.

2*d*, That chancre does not multiply itself, *i. e.* if a man is affected with a primary syphilitic sore, we never see sores of the same nature appearing on other parts of the body, unless from the application of matter from the first sore, or by contagion from another individual.

3*d*, The presence of constitutional syphilis is no hindrance to the occurrence of chancre.

4*th*, The frequency of secondary symptoms bears no proportion to the number of primary sores developed at the same time.

Numerous cases are recorded in which wounds, leech-bites, &c. assumed the characters of chancre, or were poisoned by the constitutional malady. M. Ricord has tested these cases, and relates the following interesting history in proof of the position, that "chancre does not multiply itself," &c.

A woman had on the vulva numerous chancres, which were in the period of increase, and secreted abundantly. She was seized with rheumatic pain in the right external malleolus, and leeches were applied. A few days after, she complained of much pain in the leech-bites, and on examination it was found that some were inflamed, and had assumed the appearance of the pu ecthyma. These pustules were soon succeeded by ulcerati senting all the characters of true chancres, and it was beli those attending the hospital, that they were the result of infection.

M. Ricord directed that leeches should be applied to bot and every precaution used to prevent the contact of contagi ter; and then performed the following experiments. H lated matter taken from the vulvular chancres, and matter by the ulcerated leech-bites, both which gave rise to true c The bites which had been protected cicatrized in the usu

It sometimes happens that, when leeches are applied to foul ulcerations occur, which cannot be attributed to the of contagious matter. In this case one of two things has o either the bites have been irritated, and succeeded by a furunculus which suppurates, in which case the matter is culable; or the infection has been communicated from wit wards, that is, the matter of the *glandular chancre* has in the leech-bites in its progress towards the surface, in wh the ulcerated bites furnish an inoculable secretion.

TABLE of experiments with the secretion of primary sy
1834–1837.
Male Ward.

Symptoms, the inoculation of whose secretion gave rise to the characterisic pustule.	Chancres in the period of progress,	of the penis,
		of the anus,
		of the urethra, (larvés,)
		of the lips,
		of the throat,
		of different localities,
	Primary pustules,	of different localities, the result of impure se intercourse,
		on the thigh, the result of artificial inoculation
	Virulent abscess or encysted chancre,	of different localities,
	Symptomatic lymphatitis, or chancre of lymphatics,	inoculation practised in the they were opened, or some t afterwards,
	Symptomatic buboes or glandular chancres	inoculation on the day they were opene
		inoculation practised one or more days they were opened,

N. B.—Of these latter 214 were inoculated the day they were without any result.

TABLE of experiments with the secretions of primary symptoms, 1831-1836.

Female Ward.

Symptoms, the inoculation of whose secretion gave rise to the characteristic pustule.	Chancres in the period of progress,	of the vulva,	139
		of the vagina,	2
		of the neck of the uterus,	12
		larvés (hidden chancre,)	6
		of the anus,	28
		of the lips,	4
		of the throat,	2
		of different localities,	6
	Primary pustules,	consequent on impure sexual intercourse,	27
		consequent on artificial inoculation,	
	Virulent abscesses,	of different localities,	8
	Symptomatic buboes,	inoculation, day of opening,	21
		do. one or more days after they were opened,	46

N. B.—Of these latter 20 were inoculated the day on which they were opened, without any result.

Gonorrhœa.—Many years have not elapsed since the doctrine prevailed, that chancre and gonorrhœa are the same disease, differing only in form. Hunter maintained this opinion, and accounted for the difference of form by the difference of seat; but that these diseases are distinct in nature, and very different in importance, was first taught by Sigwart, John Clement Tode, and Dr A. Duncan Senior, and afterwards demonstrated by Benjamin Bell, in his Treatise on the Venereal Disease. Although this doctrine now prevails, daily observation furnishes cases which puzzle the practitioner, and cause him to waver in the faith which he professes. Some men contract chancre from intercourse with females who exhibit only symptoms of gonorrhœa; while others are seized with symptoms of gonorrhœa after connection with individuals who are affected with chancre, and in whom this symptom may or may not be apparent. It is by no means rare to meet with cases of constitutional syphilis in persons who refer the symptoms to a previous gonorrhœa, or who disclaim having ever been affected with any primary venereal disease. But these cases can be explained in a manner confirmatory of the doctrine established by M. Ricord, that chancre alone can produce chancre. Every experimenter is satisfied of the fact, that, in the vast majority of cases, the inoculation of gonorrhœal matter is innocuous; but it is equally true, that the secretions furnished by the urethra of the male and vagina of the female are sometimes inoculable, and give origin to true chancres. Superficial observers would conclude from this, that the matter of gonorrhœa is sometimes inoculable; but M. Ricord has demonstrated the contrary, and shown that the matter of gonorrhœa *per se* never gives origin to chancre.

We here insert the table of his researches on this subject.

TABLE of Inoculations performed with the secretions of venereal symptoms not syphilitic, 1831—1837.

Symptoms, the inoculation of whose secretion was succeeded by no positive result.	Buboes occurring as the first symptom (d'emblées,)		39
	——— sympathetic,		248
	Acute Gonorrhœa	of the glans and Prepuce,—Balanitis,	28
		urethral,	291
		vaginal,	82
		vulvular,	31
		uterine,	27
		anal,	36
		ophthalmic,	6
	Chronic gonorrhœa of different seats,		112
	Suppurated Epididymitis,		3
	Symptoms not characteristic, which may succeed venereal affections, either simple or virulent.		
	Vegetations, ulcerated and not ulcerated, of different forms and localities,		28

From such data we should be entitled to conclude, that, when the matter of gonorrhœa gives origin to chancre, something more than gonorrhœa exists. But that this fact may be incontestibly established, M. Ricord demonstrates it.

1*st*, He has proved by experiment, that the matter of chancre remains inoculable when mixed with the products of secretion, morbid as well as normal, viz. urine, the mucus of the vagina, the muco-purulent matter of urethritis, balanitis, and vaginitis, saliva, fæcal matter, sweat, and spermatic fluid.

2*d*, He details cases of chancre situated on the glans or elsewhere, and accompanied by urethral gonorrhœa, in which inoculation of the secretion of either symptom indicated the virulence of the first disorder, and the non-syphilitic nature of the second.

3*d*, He records cases in which the urethra and vagina furnished inoculable secretions, and where he found, by induration, &c. in the one class of cases, and ocular inspection, by aid of the speculum vaginæ,* in the other, that the urethra and deep parts of the vagina were the seats of chancre.

We shall here detail two of the numerous cases recorded by our author, to serve as specimens of his method of experimenting.

" *Chancre on the Neck of the Uterus, accompanied by Vaginal Gonorrhœa.*—Haul. Catherine, aged 23, admitted April 8, 1834. This patient had nearly recovered from a former infection, seven months ago, when she again exposed herself, and contracted chancre and gonorrhœa from a person who was the subject of chancre only. She has not been subjected to any treatment. It is worthy of remark, that she has long been affected with chronic discharge, which determined the appearance of gonorrhœa, in those who had connection with her; but if these latter continued their connection with her after being cured, they resisted the contagion by a species of habituation.

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* For a detailed account of M. Ricord's researches with this instrument, vide Mémoire sur quelques faits observés à l'Hôpital des Veneriens. Par P. Ricord, inseré dans le 2e fascicule Tome 2e des Mémoires de l'Academie Royale de Médecine.

At present there is a chancre on the left labium, and another on the corresponding nympha. By examination with the speculum, the vagina is found to be the seat of puriform discharge;—an opaline secretion proceeds from the uterus, and on the anterior lip of the *os uteri* is situate an ulcer, of a gray colour, and with raised irregular edges.

18th. The acute stage has disappeared under the influence of emollient injections and opiate cerate; the discharge is whiter and less abundant, but the ulceration on the neck of the uterus retains its former aspect. Both thighs inoculated; the right with matter taken from the surface of the sore, and the left with the muco-purulent matter found in the *cul-de-sac* formed by the mucous membrane, where it is reflected from the vagina on the neck of the uterus.

" 19th. The points of inoculation red and elevated.

" 20th. Well-formed vesicles on both thighs.

" 22d. Pustules; full of matter.

" May 1st. Well characterized chancres in the parts inoculated; the edges of the sores are smooth and perpendicular, (*taillés à pic,*) and the surface of a grayish colour Chancres cauterized and dressed with the calomel and opium ointment. The chancres of the labium and nympha are in the stage of reparation. The ulcer on the neck of the uterus is much diminished in size, and what remains is granulating. It has been cauterized six times.

" 10th. Chancres of inoculation covered with fleshy granulations; they are indurated at the base.

20th. Gonorrhœa and chancre of uterus have disappeared. Pills of the proto-ioduret of mercury, &c. were ordered with a view to remove the induration.

" 30th. No ulceration; induration nearly gone.

" June 7th. Discharged cured.

" *Gonorrhœa : Chancre of the Urethra (Chancre larvé.)*—Br. ——, aged 19, admitted 9th of March 1833. This patient perceived a slight purulent discharge from the urethra, three days after a suspected coitus; the discharge gradually increased in quantity; he experienced acute pain during micturition; inflammation of the glans with partial phimosis took place, and, although there are some red spots, there are no excoriations.

" 11th. Gonorrhœal matter inoculated on the right thigh by three punctures.—Camphor and opium pills and emollient baths prescribed.

" 15th, The characteristic pustule has resulted from the inoculation, but it is not well developed. Inoculation repeated on the left thigh.

"17th. Characteristic pustules have appeared on both thighs. The chancres on the right thigh extend to the whole thickness of the skin. On pressing the urethra in the situation of the *fossa navicularis*, induration can be felt, indicating the presence of a chancre.

"22d. Bubo in each groin, which were leeched. The secretion from the urethra has inoculated an excoriation near the frenum; the prepuce has become œdematous; phimosis complete.

"17th. (27th?) Pills of the proto-ioduret of mercury were ordered, with a view to remove the induration; and the gonorrhœa was treated with acetate of lead injections.

"April 6th. The frenum is nearly destroyed by the chancre of inoculation. The discharge from the urethra is much diminished in quantity, and has lost its green colour.

"10th. The patient complains of tenderness of the gums; no ptyalism; gums touched with hydrochloric acid. Inoculation repeated on the left thigh with matter from the urethra.

"18th. No result from the inoculation; induration of urethral chancre is diminished; the chancre has arrived at the period of reparation.

"May 1st. Chancres on the thighs have healed; the discharge is nearly gone.

"3d. All induration has disappeared; the chancre of the frenum is cured. Mercurial treatment suspended.

"14th. Discharged."

These cases speak for themselves; but we cannot help asking how the chancre on the frenum would have been accounted for a few years ago.

Bubo.—It was a question much agitated in former times, whether bubo should be regarded as a symptom of syphilis, that is, whether it can be a source of general poisoning.

M. Ricord's experiments and observations have led him to the following conclusions on this subject.

1st, Bubo may be the result of simple inflammation, which arises either sympathetically in the part affected, or by the gradual propagation of the inflammation, whether the primary lesion be gonorrhœa or chancre.

2d, It may be virulent, that is, due to the direct application of the poison by lymphatic absorption. This kind is the legitimate consequence of chancre, for chancre alone can produce it.

3d, It may be superficial or deep, or both at the same time.

4th, It may be situated in the cellular tissue, in a lymphatic vessel, or in glands; or in the cellular tissue and a lymphatic vessel; or in all three at the same time.

5th, It may be acute or chronic.

6th, It may be preceded by other symptoms, or be itself the first.

7th, When it is preceded by other symptoms, it may either immediately succeed these, and constitute what is called chancre by succession; or it may occur when the disease has become constitutional, and form a secondary symptom.

Bubo has frequently been subjected to the test of inoculation, both before and since M. Ricord published his researches. The test sometimes succeeded, but more commonly failed, and hence it has been decried as a method of diagnosis of no value. But "this pretended infidelity of inoculation is precisely the circumstance in which its absolute value as a means of diagnosis consists."

According to M. Ricord's experiments, the symptomatic bubo alone furnishes an inoculable secretion. But in order that bubo may supply a specific purulent matter, it is not enough that it has been preceded by chancre; it must be the result of irritation from the absorption of matter from that chancre, and not a simple sympathetic irritation. Bubo from absorption is situated in the superficial glands; and although those more deeply situated may be inflamed and tumid at the same time, and even advance to suppuration, the inflammation, of which they are the subject, is simple, and altogether different from the specific inflammation, with which the first mentioned superficial glands are affected.

"It was some time," says M. Ricord, "before I discovered the reason why all buboes are not inoculable; why a bubo which does not furnish an inoculable purulent matter to-day, supplies it to-morrow; and why, in multilocular buboes, we find an inoculable matter at one part and not at another. I learned to experiment with greater precision; I inoculated the matter which first escaped on opening the bubo, and generally without any result; I then inoculated with matter taken one or more days after the opening was made, which frequently gave rise to the characteristic pustule. In many cases the inoculation continued innocuous, and I found that all those buboes, which did not supply an inoculable secretion, followed the course of simple phlegmon, and advanced towards a cure, while those which secreted an inoculable purulent matter speedily assumed the characters of chancre.

"But it might be argued that those buboes which at first furnished a secretion which was not inoculable, acquired the power of secreting a specific pus, by the application of matter from the chancre or some other cause. A case occurred which explained this difficulty. A patient came under my charge with a large suppurated bubo connected with chancre. I opened the abscess, and after having evacuated the pus from the cellular tissue, I found an enlarged lymphatic gland in the middle of the cavity, which presented signs of fluctuation. I punctured the gland, and inoculated at the same time the matter which escaped from this latter, and that which flowed from the surrounding cellular abscess. The result proved that the difference in the result of these experiments depended on my not looking for the virulent matter where it is to be found.

"After this I instituted a series of experiments which prove that inoculation is not a fallacious test. I made choice of buboes which were well advanced, and were preceded by chancre; I inoculated with the matter which escaped on their first being opened, and again with that found at the bottom of the cavity. The first inoculation was innocuous, while the second gave rise to the characteristic pustule."

Such is M. Ricord's method of research; and his experiments fully explain the cause of the conflicting testimony of authors touching the origin of buboes, and inoculation as a test of their nature.

Bubo is frequently the first and only symptom complained of, and is generally observed about a fortnight after the suspected coitus. Inspection commonly proves that a chancre does or has existed; but cases sometimes occur in which bubo is in reality the first symptom, constituting what our author denominates *bubons d'emblées*. In such cases we find that the deep-seated glands* are first affected, that their progress is chronic, and that they evince little tendency to suppurate.

The most important observations made by M. Ricord on this subject are,—

1*st*, When the *bubon d'emblée* suppurates, the matter, according to his experiments, is never inoculable.

2*d*, He has never found it succeeded by symptoms of secondary syphilis.

There is room to doubt, therefore, whether *bubon d'emblée* is determined by the fact of the coitus being impure.

From all his experiments, M. Ricord concludes,

1*st*, That virulent bubo, or bubo resulting from the absorption of the matter of chancre, is identical with chancre in nature, and differs from it in form only.

2*d*, That the symptomatic bubo is the only inoculable species.

3*d*, That all the signs indicated by authors as characteristic of virulent bubo, only serve to establish a probable diagnosis, and that inoculation alone is an unexceptionable test.

4*th*, That if a correct diagnosis is essential to guide us in our prognosis of suppurated *bubon d'emblée*, we should never neglect to inoculate its secretion at every stage of its progress; for observation has demonstrated, that buboes which are not inoculable, when the experiment is well performed, are never succeeded by secondary symptoms, and, therefore, that they are not syphilitic. Besides the syphilitic poison, other causes, which often escape our notice, may give rise to inflammatory obstruction of the lymphatic system of one part of the body as well as another, and, therefore, it would be absurd to pronounce a bubo to be syphilitic because it has occurred a short time after coition; the more so, as at a certain age there is no disease which may not be preceded by this act, so often performed, and so frequently suspected.

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* M. Ricord observes that the symptomatic bubo is situated in the superficial glands; that virulent inoculable purulent matter never passes the first gland by direct absorption; and that when deep glandular swellings suppurate they never afford an inoculable secretion until they have been infected by matter from a chancre or diseased superficial gland.

Mucous Tubercle.—This symptom, though reputed to be primary, has never, in M. Ricord's experience, supplied an inoculable secretion. "The morbid matter it secretes has been inoculated with the lancet, applied to blistered surfaces, rubbed upon denuded parts, retained on portions of the skin from which the hairs have been newly plucked, and in every case without effect."—"It seems to constitute a sort of transition between chancre and constitutional syphilis."

Chancre is the regular and constant antecedent, the specific cause of mucous tubercle; it always originates from chancre either in the infected or infecting individual. All persons are not susceptible of it; the mucous membranes are its ordinary localities, and the skin is only liable in certain parts, as in the vicinity of the anus, behind the ears, around the umbilicus, &c.

It is often difficult to distinguish mucous tubercle from chancre in the state of vicious reparation.

Every body knows that mucous tubercle is more frequently a secondary than a primary symptom, and if we consider that it occurs most frequently in women and infants, in whom the chancre to which it succeeds may remain hidden or unperceived; that it appears at a time after the occurrence of chancre, when true secondary symptoms may be developed; the genuine origin of the symptom will be admitted to have been chancre, and that it is chancre undergoing a vicious reparation or transformation *in situ.* Mucous tubercle is a common constitutional symptom, and it never is consequent on simple uncomplicated gonorrhœa.

M. Ricord concludes;

1st, That mucous tubercle is not inoculable.

2d, That it should be regarded as a secondary symptom, or an evidence of constitutional disorder.

3d, That its secretion can act as an irritant, and determine inflammation in parts with which it comes in contact.

4th. That when chancre is communicated by an individual affected with mucous tubercle, some other specific affection existed at the same time.

5th, That, like other secondary symptoms, mucous tubercle is only hereditarily transmissible.*

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* Mucous tubercle is frequently seen on the external parts of the vulva, fourchette, and in the vicinity of the anus of women who are affected with chancre; and the author of this article has seen some cases which appeared to warrant the opinion, that mucous tubercle may originate from the application of matter from a chancre pas-

Constitutional Syphilis.—Hunter proved that constitutional syphilis is not inoculable. M. Ricord has experimented with every morbid secretion which are reputed to result from syphilitic poisoning, and confirms the doctrine propounded by the great Scottish physiologist.

This subject admits of demonstration. The virus of chancre may be absorbed by the lymphatics, and remain inoculable until it has passed the first gland in relation with these vessels; but it is remarkable that, although the veins likewise absorb, we can never find an inoculable matter in these vessels, however close to the chancre we make the search. The virus ceases to be inoculable immediately after it is blended with the circulating fluid.

It has been remarked that, in cases of chancre of the glans or prepuce, the dorsal veins of the penis are liable to inflame; but it will be found that the appearance which gives rise to this opinion is, in reality, due to inflammation of a lymphatic, for, the hard swelled cord (which is frequently knotted) extends from the chancre to the glandular region without ever passing it, and that its progress is not so uncertain as that of phlebitis. When the part suppurates and is opened, the pus is never mixed with blood. M. Ricord has had opportunities of examining this lesion anatomically; and the information thus obtained confirms his statement, that the appearance in question is due to inflammation of a lymphatic vessel, and not to phlebitis.

The majority of observers agree that secondary syphilis is not contagious; but many have been deceived by symptoms which from their situation, and the time of their developement, might be referred to the inoculation of a constitutional sore. M. Ricord has met with primary ulcerations of the lips, tongue, anus, and even of the pharynx, contracted by the direct application of matter from a primary sore, and which consequently furnished an inoculable secretion. Many persons have been puzzled to account for the presence of chancre on different parts; but when such diseases as itch or prurigo coexist with chancre of the penis, we can easily understand how the disease may be propagated by inoculation to other parts of the body. Superficial observation and inquiry thus expose us to deception, for these chancres or primary

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sing into the stage of reparation, and thus constituting a symtom, strictly speaking, neither primary or secondary. The tubercles were ulcerated or not, according to the moisture of the part, and frequently disappeared spontaneously under the influence of repeated ablutions, and guarding against the application of morbid secretions.

Though the Editors are unwilling to obtrude their opinion, where a person so experienced as M. Ricord has deemed it proper to refrain, they think it requisite to say, that the tumour denominated *Mucous Tubercle* is, in all cases, merely inflammation, acute or chronic, of one of the muciparous follicles, in consequence of the application to its aperture of foul secretions or syphilitic or gonorrhœal matter. The application of any of the secretions specified induces swelling of the follicle, obstruction of its orifice, and consequently more perfect and general swelling: and this obstruction and enlargement gives rise, we conceive, to the phenomenon denominated Mucous Tubercle.—EDITORS.

pustules might be taken for pustules of ***ecthyma syphiliticum***, and the ill appreciated event quoted as a proof of the inoculability of secondary syphilis.

M. Ricord has made numerous experiments with the matter of secondary symptoms, occurring both during the continuance of the chancre to which they owed their origin, and at greater or less intervals of time, after the disappearance of the primary sore. Of these experiments we here subjoin tables.

TABLE of inoculations with the secretions of secondary symptoms, 1831–37.

Symptoms, whose secretion were found not to be inoculable.	Symptom of transition.		
	Chancres in the period of reparation,		62
	Secondary syphilis.		
	Mucous, tubercles, &c.	of different localities,	221
	Ecthyma syphiliticum,		10
	Rupia,		9
	Ulcers (consequent on mucous tubercle, ecthyma, rupia, impetigo, &c.)	of the nasal fossæ,	19
		lips,	14
		palate,	4
		throat,	81
		anus,	41
	Tertiary symptoms.		
	Tubercles, ulcerated, occupying the whole thickness of the skin,		21
	Tubercles of the cellular tissue in the state of abscess and ulceration,		11
	Periostitis suppurated,		15
	Caries of different bones,		10

TABLE of inoculations with the secretions of symptoms not syphilitic, 1831–37.

Comparative researches. Inoculation, negative result.	Atonic ulcers of the limbs,		6
	Ecthyma simplex,		5
	Herpes,		4
	Ulcers, scorbutic,		2
	scrofulous,		6
	Caries, scrofulous,		4
	Stomatitis, ulcerous,		8
	Eczema, intertrigo, ulcerated,		2
	Ostitis,		2
	Cancers,	of the uterus,	5
		rectum,	6
		breast,	2
		penis,	3
		nose,	4
	Abscess of different localities,		15

About the same time that M. Ricord's work was given to the public, there issued from the English press a book in which a doctrine, contrary to that promulgated by our author, is maintained. The work to which we refer is that by Dr Colles on the Venereal Disease, in which the doctrine is upheld that secondary syphilis is contagious.

We do not think that Dr Colles has established his position; and we proceed to show how the cases he adduces in proof of it do not militate against the doctrine propounded by M. Ricord, but admit of a confirmatory explanation.

At the 13th page of his work, Dr Colles relates the following case.—"Many years ago a young surgeon of my acquaintance, paying his addresses to a young lady, had unfortunately at the time a secondary venereal ulcer on the lower lip. The lady contracted an ulcer on her lip, which was soon followed by an enlarged lymphatic gland under the lower jaw; the ulcer had the character of venereal so strongly marked, that the case was pronounced to be syphilitic, and she was directed to use mercury." This lady died shortly after from the combined effects of mercury and distress of mind, acting on a constitution of great delicacy.

We are here informed of two circumstances, one, that the gentleman had a *secondary* sore on the lip, and, the other, that the lady contracted *chancre* from contact with that sore. Granting that the sore communicated was primary, the sore on the gentleman's lip must be shown to have been secondary, and not primary, before it can be received as evidence of the doctrine, that a secondary sore can give rise to the primary disease.

The next case is more fully stated. An accoucheur received an injury of the finger, by the falling of a window-sash; he continued to practise his profession; a sore appeared at the injured part; this sore was brought in contact with the genital organs of certain ladies; and, from this contact, *primary* syphilitic sores originated. It is further stated that this gentleman had contracted chancre on the finger at a former period. From all this series of facts we would infer, that the accoucheur had unfortunately contracted chancre again, and this the rather when we are told that his finger was in the most favourable condition for receiving the contagion of syphilis. The argument adduced by Dr Colles hinges on the fact of the gentleman having been the subject of syphilis at a previous date; but it is not mentioned that he was *not* exposed to infection. A sore is not secondary, merely because it has the appearance of being so, and occurs in company with a syphilitic eruption.

The next case adduced in proof of the inoculability of secondary syphilis, is that of a "nipple-drawer," who contracted an ulcer on the tongue, and continued the practice of her calling during the time of its persistence. It is stated that she communicated disease to several ladies of distinction; and the following are the appearances observed.

"The nipple first became slightly inflamed, which produced an excoriation, with a discharge of thin liquor; from thence red spreading pustules were dispersed round it, and gradually spread over the breast, and, where the poison remained uncorrected, produced ulcers. The pudenda soon after became inflamed, with a violent itching, which terminated in chancres, that were attended with only slight discharge; and in a short time pustules were spread over the whole body." * * * "The husbands of several had (contracted) chancres, which quickly communicated the poison, and produced ulcers in the mouth, and red spreading pustules on the body; but such of them escaped as had timely notice of the nature of the disease, before the pudenda were affected. Some infants received it from their mothers, and to the greatest part of them it was fatal." This case is extracted by Dr Colles from the 3d volume of the Edinburgh Medical Essays and Observations.

We do not consider this a distinct history of a case of syphilis; but, waiving all objection on this head, we shall start with the data allowed us, that the sore on the nipple-drawer's mouth and those on the nipples of the ladies were *chancres.*

The order of events was as follow; chancre on the nipple; *pustules* on the breasts, which became ulcers; an inflammatory itching state of the pudenda, which terminated in chancres attended by a scanty discharge; and, finally, a general pustular eruption. Several of the husbands contracted *chancres* from contact with the pudenda of their wives, and in all cases the same pustular eruption supervened. This is a curious circumstance, for the pustular form of eruption does not, for the most part, quickly follow the primary symptom.

The local nature of the constitutional symptoms, the red spreading pustules appearing first round the nipple, and being *gradually* dispersed over the breast, finally becoming sores, would, on the supposition that the original sore was chancre, lead us to consider these pustules as primary, and style these *chancres by succession*. Further, we are told that the genital organs became itchy and inflamed, and that chancres or sores appeared thereon. It would suffice, were we to show that a doubt exists concerning the secondary nature of the sores on the pudenda, for no doubts should shadow facts, subversive of a principle fixed on so broad a foundation as that propounded by M. Ricord. But more than a doubt exists; for it is not only possible, that the poison of syphilis may have been conveyed by the patients themselves from the nipple to the pudenda, but it is highly probable that women who have been lately delivered, and in whom the breasts and genital organs are so circumstanced as to demand more than common attention, would communicate a contagious disease of one organ to another. Thus we would not think, that if the nipple-drawer contracted *chancre* during the exercise of her profession, she had received the infection of constitutional syphilis; for in the person from whom she received it the order of events was merely changed; —the disease had been conveyed from the pudenda to the nipple. We consider, then, that this history does not tend to prove that contact with a secondary ulcer is equally to be dreaded as contact with a primary sore; that *ecthyma syphiliticum* is identical in essence with chancre; that secondary syphilis can give rise to the disease of which it is a consequence.

With regard to children born syphilitic infecting their nurses, the question is more intricate. That syphilis, as a constitutional malady, is hereditarily transmissible from parent to offspring, is a fact familiar to every medical observer; but that children who are the subjects of constitutional syphilis alone can communicate chancre is very apocryphal, and contrary to what we positively know of the natural history of the disease. Experiment and observation have amply proved that the secretion of a secondary pustule, abscess, or ulcer is not inoculable,—that the purulent matter is not specific; and, therefore, when a child primarily infects its nurse, we have a right to doubt whether that child was not the subject of chancre. When we consider that the presence of the secondary disease does not forbid the inoculation of the primary form, and that chancre is frequently situated in the deep parts of the vagina, we can readily understand, how a child which inherits the constitutional disease, may have the primary form superadded, and how a nurse infected by such an infant may be supposed to have received the infection of secondary syphilis. When it is further remembered, that the venereal disease has but lately commenced to be understood in its details, and that our judgment of the constitutional evidences of the presence of the disease is for the most part founded more on partial examination, and on the history of the case, than on accurate inspection of these evidences, we can readily perceive how a small chancre may escape detection amidst the eruption which marks the constitutional disorder, and, consequently, how we may be deceived touching the contagious nature of secondary syphilis.

Hunter not only denies that nurses can be contaminated by infants secondarily affected, but asserts that children cannot receive the constitutional disease from their parents; for, according to his experiments and observations, secondary syphilis is not inoculable; and if contamination from it could once take place, "it would be possible to contaminate for ever." He makes some observations on the nature of secondary syphilis, and refers it to the diffusion of the venereal poison through the circulating fluids, forcing, as it were, " certain parts of the body to assume the venereal action, which action is perfectly local, and takes place in different parts, in a regular succession of susceptibilities." This theory implies the indefinite circulation of a poison, for years frequently elapse between its absorption and the appearance of constitutional evidences of the event. It likewise implies that these evidences should be chancres, the symptoms being produced by the actual application of syphilitic matter to the parts in which the disease is manifested. But secondary pustules, &c. are demonstrated not to be chancres, and therefore secondary syphilis, though dependent on the absorption of the matter of chancre, is a disease *sui generis*, having no symptom in common with that which it recognizes as its cause. It appears that the circulating fluids are contaminated by the poison of chancre; that the irritability of the capillary system is modified by its stimulus being changed; that the disease is dependent on a poison having circulated, and not on a poison circulating. The irritability of the capillary system being modified, common exciting causes become morbid stimuli; and thus we find ordinary exposure determining ulceration of the throat instead of slight cynanche; and a blow on the shin giving rise to ostitis in place of slight temporary pain and discoloration. Amidst the multitude of affections termed secondary, we find none which bears any analogy to chancre, either in property or appearance. We vainly inoculate the matter of abscesses, secondary buboes, ecthyma, rupia, ulcers, &c. from which we conclude (logically, it is conceived,) that they are different, distinct from chancre, which, when properly inoculated, always gives rise to chancre. The matter of these affections may be specific; but as yet we are ignorant of what constitutes their specificity; and before cases like those we have reviewed can be received as evidence of the contagious nature of secondary syphilis, it must be shown that the sore on the accoucheur's finger could not have been chancre; that the sores on the pudenda of the ladies were not chancres; and that the children referred to were not the subjects of chancre. All this must be proved beyond a doubt, before the doctrine can be set aside, that "chancre alone can produce chancre;" and the converse established, that "secondary venereal sores can produce primary ulcers."

We have thus endeavoured to show, that the arguments advanced by Dr Colles in favour of the doctrine he professes do not establish his position; but, at the same time, we confess that cases are recorded, and have even come within our own knowledge, which cannot be so easily explained. Benjamin Bell relates, at the 426th page, Volume 2d, of his work on the Venereal Disease, the following fact:—"About ten years ago I was desired to visit a child seven or eight days old. It was covered with a rash, which had much the appearance of being venereal; and finding that the only other child which the parents ever had was born with a similar rash, of which it died, I inquired at the father whether there was any cause to suspect that he was affected. He informed me that he had been poxed about six months before his marriage, and that his symptoms were chancres and sore throat; but that, having taken as much mercury as was judged to be sufficent, the symptoms having disappeared while under the course, and none having occurred again, although he had now been married three years, he could not possibly believe that the child was affected with this disease, particularly as no symptoms had appeared upon his wife." —"Two nurses were infected by the suckling of this child. The first became so much distressed with ulcers on her nipples, and pains in one of the mammæ, that she was obliged to leave the family; and although warned of her situation, and of the necessity of giving no suck to other children till the course of mercury she was put under was finished, she foolishly took home her own child, and in the course of two or three weeks he also was poxed." —"The nipples of the other nurse ulcerated, and she was soon thereafter seized with a venereal ulcer in the throat," &c.

The case thus distinctly stated by Mr Bell is complete in its kind, and we fully perceive how it bears upon the point in question. It is as strong a proof as can be advanced, of the inoculability of secondary syphilis. But there is a hiatus in the history; we are not told that the child could not have received the infection of chancre. On reading such a case, however, and in ignorance of researches so numerous, so precise, and so uniformly demonstrative of the fact, that secondary syphilis is not inoculable, as those of M. Ricord are, we should rest satisfied with such a proof of the opposite doctrine; but a series of questions naturally arises in the mind, after we have had ocular demonstration, that while the matter of chancre is always inoculable, the secretion of secondary symptoms has never proved contagious in the experience of the same most skilful experimenter. How does this happen, if the secondary syphilis is in reality inoculable? How is it that the proofs of its contagious nature are spoken of as something rare, and not as matters of daily experience, as the language of the upholders of this doctrine would lead us to suppose they should be? Opposed as these varieties are to the unsophisticated belief of all men, as exhibited in their conduct, however in accordance with the theory they uphold,—contradicted as they are by daily experience and clinical observation,—how are we to receive them in company with the thousand facts introduced to us by M. Ricord? Surely, in the absence of opportunity to examine their real merits, we may be allowed to express a doubt whether the whole facts have been properly appreciated. Remember that chancre has been detected where it was not suspected either by the patient or his physician; and that, in contradiction to the strongly expressed opinion of such a man as Hunter, it has been demonstrated to exist in the urethra. Consider that secondary syphilis has appeared in many who were, in reality, ignorant of having ever been the subjects of the primary disease; that chancre itself has been present, and the patient stoutly denied that such could be possible. Reflect that, if it is always an object to conceal one's shame from the world, it is frequently a source of joy to hide from ourselves a truth, which would mortify and distress us. Recollect by how many quibbles and lies the physician is daily thwarted in his search after a truth, from whose exposure the deceiver himself would be the first to benefit, and then say whether a great principle like that propounded by M. Ricord, and grounded on a mass of facts, distinct and precise, must give way to a doctrine with observations few and general for its basis. We confess our inability to explain these cases; but we believe that enough will be found in this paper to warrant our emphatically expressing a doubt whether their intricacies were fully and fairly appreciated.

In fine, we cannot understand, how diseases, which are merely modified in their nature by a state of the system dependent on a cause which may have existed yesterday or ten years ago, should secrete a poison identical in essence with that to which they remotely owe their modification. Such a supposition is unsupported by reasoning, either direct or analogical.

The test of inoculation may sometimes prove valuable in medico-legal investigations.

Circumstantial evidence frequently aids in the elucidation of questions brought before the medical jurist. Cases sometimes involve questions relating to the venereal disease, and it would be difficult to decide whether we should be most surprised at the ignorance or rashness which characterize the manner in which they have oft-times been decided.

The presence of chancre is frequently adduced as a circumstance aiding in the proof of rape having been committed; and the time is not long past when the coincidence of gonorrhœa in the man, and chancre in the woman alleged to have been violated by him, would have been at once received as strong corroborative evidence of the fact. But M. Ricord inquires, "if the gonorrhœal matter secreted by the accused be found not to be inoculable, is it not evident, if his disease be recent, that the coincidence of disease in both parties ought not to be received as condemnatory evidence? Could we not, by this method of inquiry, show that persons accused of having communicated chancre, have determined only simple inflammation? Could we not, by this direct and certain mode of diagnosis, negative hurtful imputations, and remove the doubt which otherwise would remain in the mind of the conscientious physician? Such results would of themselves justify the numerous experimental researches I have made, independent of their having elucidated questions previously insoluble, and aided in the destruction of prejudices consecrated alike by authority and time."

On the employment of Mercury in the treatment of Chancre. —As the practice pursued by M. Ricord is detailed in the paper formerly referred to, we deem it unnecessary to republish his observations. We presume that the readers of this Journal are acquainted with the principles which guide him in the management of chancre generally, and are aware that indurated chancre is the only form of primary syphilis, for the cure of which he makes use of mercury. Without entering into detail, therefore, we shall endeavour to state in a few words what seems to be the state of science in reference to this most important question.

Syphilis and the whole train of morbid effects which result from the promiscuous congress of the sexes, were formerly ranged under one head; and the doctrine was established, that what is necessary for the cure of one symptom, is equally demanded for the treatment of the rest. Mercury was deemed a kind of panacea; patients were anxious and confident; physicians were generous; and rottenness and death were the frequent results of the indulgence of these sentiments.

But some men have continued to observe, the reason of things has been diligently searched for, and a degree of success has rewarded scientific efforts to discover the truth. As knowledge expanded, contracted views gave way. It was found that all the effects which result from impure sexual intercourse are not syphilitic, and the ancient doctrine was consequently condemned as being too inclusive.

The ground which was divided by the plough of the husbandman has been more thoroughly broken up by the spade of the gardener; and the consequence has been, that the dogma which declares that mercury is required for the treatment of venereal diseases, was supplanted by the doctrine, that mercury is essential for the cure of chancre, which in its turn has been succeeded by the sounder statement, that mercury is not a specific for the extinction of syphilis.

The power of this mineral has been chiefly recognized in its most striking effects, and conclusions deduced which a more perfect knowledge of the physiology of the tissues in which these effects are seen, and an acquaintance with the more delicate workings of this agent, may or may not warrant. When an opinion is formed on individual experience, subversive argument is of little avail, for self-esteem is attacked, and is interested in making a vigorous resistance; and when this opinion is backed by other individual experiences, the moral combat becomes more hopeless, because the self-estimation of many is assailed. Prejudice attaches us to the principles in which we are educated, and renders them objects of affection; and hence the obstinacy with which men cling to ideas with which they are familiar. The experience of the world has shown that partisans are sternly adhesive; that truth is more frequently imagined than perceived; that the value of a reason is too seldom accurately measured; and that minds satisfied with partial knowledge far out-number the warm conscientious lovers of philosophy. We can readily perceive the reason why men spurned the great principle promulgated by Rose and Thomson. It was a heavy blow aimed at a favourite, long cherished opinion; but the truth will stand fast, despite the efforts of the boisterous declaimer, or the insidious endeavours of the smiling sophist. Rose and Thomson demonstrated an important truth, they incontrovertibly proved that mercury is not necessary for the cure of chancre, as chancre; and this the intelligent ingenuous mind will acknowledge, however much it may dissent from their method of applying it.

Chancre, as a local disease, is of little importance, but as a source of serious disaster, it demands our most anxious attention. M. Ricord has found that the chances of constitutional poisoning are in the direct ratio of the duration of the primary disease, and that the means employed for the removal of the primary sore have no specific influence in preventing the occurrence of subsequent calamities. It becomes a question of time, therefore, and ample experience has shown that unindurated chancre yields more rapidly to local than to mercurial treatment. But indurated chancre disappears sooner under the influence of mercury than simple local means, and as rapidity of cure is our object, we have recourse in this case to the agency of this mineral. There is a compound half-acknowledged belief in the minds of many, that mercury cures indurated chancre, by virtue of a kind of antidotal property, and that the constitutional disease is less likely to originate from a sore which has been removed through its agency, than from one which has been subdued by simple means. This, however, is a mere fancy, and is not supported by accurate observation. Mercury has no specific action on chancre, whatever it may have on the constitutional effects consequent on the absorption of the poison of syphilis; and, far from guaranteeing against the occurrence of the signs of general empoisonment, it does not even modify these evidences, unless the constitution has been injured by its mal-administration.

The indication for the exhibition of mercury in primary syphilis, is to remove the induration, which is a bar to the healing of the sore. We do not deny that simple chancre may disappear under its influence, but we think that it cures the disease on a common principle,—a principle identical with that which we recognize in the cure of chancre by local stimulants. Mercury is a general stimulant to the capillary system, and it cures a local specific inflammation, by inducing a general inflammation of another kind, in which the local disease participates. The same principle is illustrated in the cure of the constitutional symptoms, for, as it is certain that these symptoms are referable to a poison or morbid stimulant having circulated, by which the irritability of the capillary system of vessels has been modified and rendered obnoxious to common causes, rather than to a virus circulating and presently urging them to diseased action, so, by virtue of its generally stimulating property, mercury excites these deranged vessels to another action, and the symptoms of uncleanness fade and disappear.

The great object with many practitioners is to induce ptyalism or mercurial stomatitis; but although these are evidences that the constitution feels the influence of the remedy, they are effects to be avoided, and not indications to be fulfilled. M. Ricord has shown that the indication for the exhibition of mercury is the removal of the induration which prevents or retards the healing of the sore. The indication no longer exists when this induration is so far removed as to cease to be an impediment to the salutary operation of local means, or when the mercury has commenced to exert a baneful influence on the constitution, as evidenced by inflammation of the salivary glands, soreness of the mouth, &c.

Much importance is justly attached to the induration which characterizes the Hunterian, the typical form of chancre; but, so far as we are aware, no one has attempted to indicate the cause, or point out the real nature of this induration. It is dependent either on simple or specific inflammation. If it depends on a specific inflammation, why are the exceptions to its presence so frequent? We believe that it occurs independently of any specific nature in the irritant, and that the inflammation which occasions it is as different from the specific inflammation of syphilis, as that which gives origin to phlegmon in the cellular tissue which envelopes a diseased gland is different from the specific inflammation of which that gland is the subject. It seems to be determined by the tissue which is the seat of disease; for, when the sore is superficial and confined to the mucous membrane, no induration occurs, whereas if the inflammation extends to the cellular tissue, lymph is effused, and we have the true Hunterian chancre. The effusion of organizable lymph is a healthy process; it is a means adopted by nature to arrest the progress of local mischief, and in it we see an additional evidence of chancre being a local disorder. If the constitution is seriously deranged, the adhesive inflammation will not take place, but the chancre will assume another character, and operate more destructively on the tissues. Induration, or the effusion of organizable lymph, is an every day effect of common inflammation, and we see no reason why in this case it should be regarded as the specific effect of a specific cause.

So far as we have seen, we are warranted in making the observation, that ulceration or suppurative inflammation are necessary effects of the presence of the syphilitic virus; and from this we would infer, that, when the cicatrization of a chancre is complete, all chance of relapse ceases to exist. We say all chance of the recurrence of chancre ceases, not the recurrence of ulceration. These remarks we offer, because it is a common belief, and one in which our author participates, that induration *per se* is a source of danger. But although ulceration may recur after cicatrization has been complete, we are aware of no facts which prove such relapse to be syphilis, except when induced by sexual intercourse; and consequently we think that those practitioners are in error who persevere in the use of mercury until all hardness is discussed. Hardness sometimes persists for years, or even for life.

In conclusion, the existence of chancre, or the supposition that a sore is syphilitic, is no reason in itself for mercury being exhibited; for experience has amply proved that chancre, as chancre, does not require the administration of this mineral either to heal itself, or to prevent its effects from taking place; and we are practically taught, that when it cures the primary disease, it does not guarantee against the occurrence of secondary syphilis. Although mercury is frequently required to aid us in the cure of chancre, we are not warranted either in rendering the present state of the patient worse than it was, or in so modifying his constitution, that affections as bad as those we wish to avert, may occur at some future period on the application of common causes. Mercury is only applicable to the cure of indurated chancre, and its exhibition should cease when the sore is susceptible of the influence of simple local remedies.

Art. III.—*On Insanity; its Nature, Causes, and Cure.* By William B. Neville, Esq. of Earl's Court House. 8vo. London, 1836.

It is not very creditable to the general character of the human race, that those species of afflictions which are calculated to inspire, in the highest degree, feelings of commiseration and sympathy, are such that their subjects cannot, with propriety, be trusted solely to the cares and attentions of the nearest and dearest of friends and relations; and hence, in all well regulated communities, the law has stept in, to take under its special protection, those individuals, who have lost the power of self-control; and are, therefore, by universal consent, deprived of the privilege of self-management. The loss of reason, that pre-eminent endowment of man, to which he owes so much of the dignity of his nature, leaves the noblest of God's creatures in a sad state of humiliation and abandonment. Not to be able to look after himself, is often the precursor of neglect on the part of others; for it is the unfortunate attendant, in many instances, of aberration of mind, to take the most pertinacious dislikes to those whose attachments are greatest; and to see interested or criminal designs in quarters where nothing had ever been previously exhibited, but the warmest affection and regard. This circumstance necessarily produces a separation from those, whose cares would otherwise be most unceasing and efficient; and if even it were not so, the harassing spectacle of loss of reason in those who are most dear to us; the want of sufficient control on the part of friends; and, the salutary operation, in a curative point of view, of an entire change of scene, and of ordinary associations, generally makes a complete removal from home desirable. It unfortunately happens, likewise, that long before the occurrence of circumstances, which place the nature of the disease beyond reasonable doubt, various bizarraries often exhibit themselves, which operate in producing feelings of estrangement.

Unfounded suspicions and jealousies may occur, of those to whom they could least apply; the general conduct may be wayward, unreasonable, and capricious; and without that consideration for those around them, for which it was perhaps ordinarily distinguished; and the alterations and irregularities which may, in fact, be the effects of disease, are often attributed to change of character and disposition. Nor is this impression always evanescent, for it not unfrequently exists long after the period when it could not but be regarded as morbid.

Under all these circumstances, removal from home, and from the inspection of friends, makes it incumbent on the State to come forward with such arrangements as are calculated to promote the comforts, and protect the interests of such as are unable to do this for themselves. The laws, therefore, which concern the management of lunatics, form a very important and interesting branch of national jurisprudence; and as they have been much changed of late years in England, and as their operation and bearings ought to be well understood by the professional public, it may not be without its use, to give a slight sketch of the principal existing enactments on the subject.

The common law of England, in contradistinction to the statute, appears to have been the principal means by which, for many ages, protection was afforded to the persons and property of lunatics; but in the Journals of the House of Commons a record is found of the introduction, on the the 3d of October 1653, during the period of the Commonwealth, of a bill on behalf of "idiots, lunatics, and infants," founded on a petition presented in the previous August on their behalf, by Peter Egerton and others. The bill was amended so as to include only "idiots and lunatics;" and after twice reading, and a dissolution of Parliament, seems to have been lost sight of. More than 100 years afterwards, namely, on the 27th of January 1763, a committee of the House of Commons was appointed "to inquire into the state of private mad-houses in this kingdom, and report the same, with their opinion thereon, to the House." After examining evidence on the subject, and receiving the opinion of Drs Monro and Battie, in favour of the enactment of some regulations for the government of private mad-houses, the committee reported, " that the present state of the private mad-houses in this kingdom requires the interposition of the legislature." This report was not acted upon till about ten years afterwards, when a bill was brought into Parliament, which, after much consideration and modification, became the law of the land in the summer of 1774.

By this statute, (which did not affect public hospitals nor houses containing one patient only,) the licensing and superintendence of lunatic establishments in London and Middlesex, were placed under the authority of the Royal College of Physicians of London, who were empowered to elect annually, for not more than three years at a time, five of their fellows as commissioners (and in defect of fellows licentiates,) none of whom should serve more than three years in succession, and two, who had acted as commissioners the previous year, should annually go out of office. The college treasurer was to be the treasurer of the commissioners, and the elects of the college were to be the auditors of his accounts; a secretary was to be appointed at a salary; the commissioners and officers were to be sworn; the commissioners were to be paid by the treasurer one guinea and their expenses for each visitation; and no commissioner could be concerned in a private establishment, under a penalty of L.50. Licenses were given, annually, by the commissioners, the sum of L.10 being paid for houses containing fewer than ten patients, and L.15 for all such as contained above that number. Visitations were to be made once a-year, or as often as the commissioners might think necessary, or the great law officers might direct; and the names of every person received into a mad-house, with certain specified certificates of admission, were to be reported to the secretary of the commissioners, within three days after admission, under a penalty of L.100.

These regulations extended only to London and Middlesex; but in all other parts of England, the magistrates at the quarter-sessions of the peace, had the power of granting licenses, under similar terms to those above stated, with the clerk of the peace, as their treasurer and secretary; and of nominating one physician and two justices, as visitors of such licensed houses in their respective districts, who were to have their expenses, and a reasonable remuneration for the exercise of their functions. The proceedings of such visiting physicians and justices were directed to be transmitted regularly to the Secretary of the London Commissioners; and information given to the same person, of the reception of every lunatic, just as in the case of the establishments in

London and Middlesex. Under this system of management, the business of private lunatic establishments was conducted till the passing of the act of 1829; but for many years previously, the laws had been found to be exceedingly inefficient and defective; and, indeed, from the time of the publication of some very important and striking reports, by committees of the House of Commons, fourteen or fifteen years before, attempts had been almost annually made, but without effect, to render them more efficient. Repeated discussion and renewed inquiry were, however, serviceable; and, at length, the perseverance and judicious management of Robert Gordon, Esq. Member for Cricklade, and now one of the Secretaries of the Board of Control, carried through Parliament, but not without many difficulties, that series of enactments, which, with some unimportant modifications, forms the present system of laws which respect the care and treatment of insane persons in England.

The licensing and visitation of every lunatic-house for the reception of two or more patients (unless it be a public hospital,) in the cities of London and Westminster; in the county of Middlesex; the borough of Southwark; and the nearer parts of Surrey, Kent, and Essex, devolve on from fifteen to twenty commissioners, who are annually appointed by the Lord Chancellor, and whose names are published in the Gazette.

Of these commissioners, not fewer than four, or more than five, are to be physicians, and two barristers; and all of them are to be sworn to the proper performance of their duties. The travelling expenses of the whole of the commissioners are to be paid; and the physicians and barristers (provided the latter are practising ones) are to be remunerated by the payment of L. 1 for every hour employed in visitations.

In the country, with the exception of such parts as are comprized under the metropolitan district, it is provided that the justices at their Michaelmas session, shall annually appoint three or more justices of the peace, and one or more physicians, surgeon, or apothecary to act as visitors of each lunatic-house, containing two or more patients. The names are to be advertised in provincial papers, and the medical gentlemen only, to be remunerated for their attendance. No commissioner or visitor is to be connected with any lunatic establishment, either as a proprietor, or regular medical attendant.

Licenses, which are to be renewed annually, are given at any quarterly meeting, by five commissioners, of whom not more than two must be physicians; and by any quarter sessions of the peace in the country: and fourteen days previously to such meetings or sessions, applications must be made for licenses, which must communicate with the other necessary particulars, a plan of the house applied for, on the scale of one-eighth of an inch to a foot;

and a statement of the greatest number of patients proposed to be admitted into such establishment.

Licenses may be revoked, or their renewal refused, by the commissioners or visitors ; but the decision is not operative, till approved by the Lord Chancellor. The expense for licenses to be 10s. for every insane patient not being a parish pauper ; and 2s. 6d. for every parish patient ; but so as that not less than L. 15 per annum, in the whole, should be paid for any license. Visitations to be made at least four times a year in the metropolitan, and three times in the other districts ; not less than three of the commissioners, in the former case, of whom one at least shall not be a physician, and by two visitors in the latter. The attendance of a medical gentleman, it may be observed, is not required in either case, it being inferred, we presume, that such attendance will always be given as a matter of course. The visits are to be made during the day, unless there is information on oath, that a night visit may, on account of some mal-practices, be desirable, when it may be given.

A power is given to the general body of commissioners, and to justices at their quarter-sessions, to liberate any person who may be considered as improperly confined ; due and specified investigation having been previously made, and proper notices given to all the parties concerned.

Before the admission of a patient into a lunatic asylum, a certificate of insanity is required from two medical gentlemen, not partners, after a separate visit from each not more than seven days previous to the period of confinement ; but when the visits of two medical gentlemen cannot, from some particular circumstances, be conveniently obtained, then the certificate of one may suffice till that of another is obtained, which must be had within seven days from such admission. No person interested in such lunatic establishment, nor its regular medical attendant, is to sign such certificate ; nor any one whose father, son, brother, or partner stands in a similar relation. Pauper-lunatics require the order of a magistrate, and the certificate of a clergyman, church-warden, and medical gentleman, to warrant admission. The Clerk of the Metropolitan Commissioners, or the Clerk of the Peace of counties, as the case may be, is to be informed within two days of the admission, removal, or death of patients ; and in the case of the death of pauper-patients, the medical gentleman is to transmit, likewise, a statement of the causes of death. Correct registers to be kept in houses, and also a plan of the premises, for the inspection of the commissioners or visitors.

Lunatic houses containing 100 patients are to have a resident physician, surgeon, or apothecary ; or when there are fewer, unless such house be kept by a professional gentleman, two professional visits are to be given in the week, and a proper entry made thereof, and of the necessary particulars relating to them.

Commissioners and visitors are directed to take minutes in their visitations, and to transmit a copy of them to the clerk of the commissioners and clerk of the peace; but such clerks are not to be permitted to attend the visitations, unless particularly required to do so. Inquiries are to be made during visitations whether Divine service is performed, or the spiritual consolations afforded; and what description of amusement, employment, or recreation is provided.

But where the number of patients is fewer than 11, permission may be given to lessen the visits to once in four weeks, instead of twice a-week, which is a reduction to the singular amount of seven-eighths of the whole attendance previously required.

These are the principal enactments of the existing act, which is enforced either by penalties, or, in case of higher delinquency, by prosecution for misdemeanour; and it appears to us that every material circumstance is very fairly provided for. There was one point, however, which, from its peculiar difficulty, impeded for some years the progress of the bill; and this was the mode in which the evils could be provided against, which arose from confining single patients in separate houses,—an arrangement which, it is clear, might sometimes be highly disadvantageous. The present act makes it obligatory on every person who takes the charge of a single insane person, (except a guardian, or relative, or committee of the Court of Chancery,) to have similar certificates to the ordinary ones required for admission into lunatic houses, under pain of being guilty of a misdemeanour; but instead of sending immediate notice of such admission, it is only required to transmit the necessary information, in case of the person not being, within twelve months, able to return to his or her usual place of abode; and thus it is to be transmitted to the clerk of the metropolitan commissioners, marked *private returns*, and only to be inspected by him, or by the Lord Chancellor or Home Secretary of State, or persons authorized by them for the purpose.

It may also be stated, that proper measures are provided for the regular medical inspection of all persons confined as lunatics, under the immediate orders of the Lord Chancellor or the Home Secretary of State. The Lord Chancellor appoints two physicians and one barrister, with salaries respectively of L. 500 per annum, and L. 800, besides travelling expenses, as visitors for the patients under his immediate jurisdiction.

Lunatic asylums for paupers are established in counties by the authority of a particular act of Parliament, under the direction and superintendence of the magistracy.

We have thus given a general account of the existing laws which respect the regulation of lunatic establishments in England; and for further details must refer to the act itself.

We proceed to the consideration of the work before us, the author of which has had much practical experience in the treatment of the insane, by having been long the medical superintendent of Earls Court House, a very repectable private lunatic establishment for females at Brompton, in the immediate neighbourhood of London. This mansion acquired much distinction in the scientific world, as having been long the residence of John Hunter, and the scene of many of those important inquiries which have so eminently distinguished him. It now affords an interesting exemplification, to which we can bear our personal testimony, of great general good management of the unfortunate inmates, and of that attentive, kind, and discriminating regard to their comfort and well-being, which is so eminently conducive to recovery. The author has appended, as a separate publication, various plans and sketches, by which an idea may be formed of the agreeable accommodation afforded at Earls Court, and of the fitness of the arrangements for the objects in view.

In some introductory observations, which occupy the first chapter, the author takes a general view of the opinions entertained relative to the nature and characteristics of insanity from the earliest periods; and he views, as objectionable or insufficient, the various definitions which have been employed to characterize that malady. The test proposed by Shakspeare, of re-wording his statements, on which Sir Henry Halford laid so much stress in one of his College papers, the author regards as altogether fanciful; and is of opinion, that " neither one nor twenty such tests can be held as conclusive."

With regard to the author's own ideas of a sufficient definition of insanity, we have them thus offered to us in his second chapter, viz. "Disordered functions of the brain generally, or of one or of several of its parts, without consciousness of this derangement on the part of the individual affected, and without notable or necessary implication of the functions of any other system in the body." Idiocy, on the other hand, (which he invariably terms idio*t*cy,) he defines to be, " deficiency of mental manifestation, from lesion, imperfect developement, or absence of one or more of the cerebral organs."

We do not, however, see the necessity of any inferences as to the immediate causes of insanity or idiocy, entering into that general announcement of their phenomena, by which the peculiar character of these maladies is to be designated. If deficiency of " mental manifestation," (by which, we presume, that the author would imply deficiency of mental or intellectual power,) is the characteristic of *idiotism*, it is not necessary to connect this with any circumstance from which it may be supposed to originate. Fever and pain of the side are two of the prominent distinctions of pleuritis; but it would hardly be deemed requisite, in the definition of that complaint, to connect these symptoms with the internal inflammatory affection from which they originate; and idiocy would still have its main features indicated by deficiency of mental powers, whether it was attended or not with organic lesion, irregular structure, or imperfect developement. Nor do we seem to gain any thing by the author's definition of insanity; for disordered mental manifestation,—a term which he often employs,—would indicate insanity, mental derangement, or any other synonym which we might use as indicative of the disease, if there were no organic change, or diseased animal action whatever. But here we should have nothing more than a change of term, without any improvement in the precision of our ideas.

Disorders of the mind are only limited in number by the diversities which exist in the faculties liable to be affected in them; but these faculties, in their division into the Moral and Active, and the Intellectual, afford a useful means of classifying the various modes in which insanity exhibits itself. Of these divisions, either separately or in conjunction, the author has availed himself in his classification of diseases of the mind, which he places under five heads.

I. General affective and intellectual insanity	When acompanied with violence or exaltation, entitled *Mania* by writers generally. When unattended with violence, generally spoken of under the title of *Dementia*.
II. General affective insanity	
III. General intellectual insanity	
IV. Partial effective insanity	Generally taken in conjunction under the common name of *Monomania*.
V. Partial intellectual insanity	

In this division, we presume, that the term *affection* is meant to imply a state of disease, in which the *affections*, comprehended under the moral and active powers, are concerned, though precision would have made it desirable for an explanation to have been given on this point. The author is of opinion, that to constitute insanity, there is no necessary derangement in the functions of any other system, except the brain; but still it very rarely happens, he informs us, that the functions of organic life, namely, digestion, circulation, respiration, or secretion, are not in some degree affected, though in an adventitious, or accessory manner.

In the early stages of the malady, the tongue is often "furred, the mouth dry, the breath unpleasant, the appetite and the bowels obstinately constipated." To these are usually added some of the ordinary symptoms of fever; but in general the whole of these phenomena speedily disappear, and leave the mental affection alone remaining.

Want of sleep is one of the most common and troublesome accessory symptoms of insanity; and a return to good nights is among the most auspicious and favourable circumstances which can attend it. Increased or diminished sensibility of skin are very common attendants, as is likewise a sense of burning heat in some region of the body, and sometimes a feeling as if things touched were soft or yielding, hot or cold. Insensibility to great heat, or great cold, particularly the latter, have been often observed; but it is a serious error to imagine, that the insane are all of them usually insensible to a low temperature, for some, on the other hand, shrink under its paralysing effects, and shiver under the slightest exposure. Muscular power is, for the most part, increased, but it is occasionally much diminished; and such cases generally terminate in paralysis, or something approaching to it.

The external senses seldom, with the maniacal, escape some degree of derangement, and their perversion may lead to the use of disgusting articles of food, or to the imagining of bad smells from the simplest and most agreeable diet. It is not, however, always the external organs that are, in such cases, morbidly disposed. The false perceptions, now mentioned, are sometimes owing to internal disorder in the nerves distributed to the organs of sense; or they may be of a mixed nature, arising, on the one hand, from the " part within, which perceives impressions," and, on the other, from " the instrument without, which receives them."

The natural attachments and affections occasionally become exceedingly altered, in insanity, the modest becoming indecent; the attached, estranged; the confiding, suspicious. But, at the same time, Pinel has observed the reverse to be sometimes the case; and the author believes him to be perfectly correct, when he states, that " nowhere, save in romances, met he with more tender husbands, more affectionate parents, more devoted lovers, or more disinterested patriots, than in establishments for the treatment of mental derangement."

Consummate address in concealing their real state, or deep cunning in their actions generally, are not uncommon with maniacs; nor is vanity, in reference to appearance and decoration, particularly among females; suppositions of prophetic endowments, and ideas of superhuman sanctity.

With regard to the *intellectual* faculties, numerous are the modes in which they can be affected, either in the way of perversion, excitation, or depression. Mr Neville considers the disordered faculties of the mind, as combining and producing " a variety of results, in the same manner as the healthy powers," and with the facility and confidence of a phrenologist, regards the various symptoms which exhibit themselves, as disordered manifestations of different organs, which may mutually influence each other. In this way, he is of opinion, may be produced alternations of "fury, of apathy, of pride, of humility, of vanity and indifference, of a mischievous disposition, or gentleness, just as it may happen that the particular part of the brain on which those faculties or attributes depend, may be morbidly affected."

The powers that constitute the mathematician may be exhibited in "high activity," and we presume likewise in corresponding depression; the estimate of time may be singularly perverted; and the musical faculty may be deadened or excited, so as that those who have been remarkable for no musical talent whatever, may "suddenly display singular accuracy of ear and taste," and the musical, lose all pleasure from harmony, or of pain from discord.

It would be a curious subject of remark, whether, in the former part of the last mentioned category, a person who has not a musical ear, that is, who cannot detect a voice, or an instrument being out of tune, should obtain the faculty of accurate discrimination in this respect, through the medium of disease. A fair test of this circumstance would be, that an individual who could not sing in tune, or dance in time, when well, should be able to do both correctly when deranged;—a point which we exceedingly doubt.

In his third chapter, the author treats of the *predisposing causes of insanity.*

One of the most powerful of these is hereditary disposition; though this may be taken in too extended a sense when applied to the faculty of transmitting a disease, whose origin may be personal. Idiocy is common in early life, and often connected with precocity of talent; but insanity is hardly known in infancy; is most frequent between twenty and forty, and very rarely occurs after sixty years of age.

The author considers the state of mind of the mother during pregnancy, as having an important influence in producing malformation in the fœtus; and of thus giving rise to a disposition to spasms, convulsions, and other nervous affections in childhood, and, in youth, to madness or imbecility. M. Esquirol states, that it is often in the womb we are to look for predisposing causes of mania and imbecility; and that, during the great agitations of the French Revolution, (and the same has been observed relative to the Irish Rebellion and the American disturbances,) a vast number more of paralytic, idiotic, and unhealthy children were born, than at any other period. An instance came within the author's own knowledge, in which a much attached couple were obliged to be separated, from some family circumstances, after many years of a happy union. In a few weeks the wife received the abrupt and melancholy tidings of her husband's sudden death. She was was then pregnant, and, on delivery, a few months afterwards, when she had a good deal recovered her quiescence of mind, the child was found to be diminutive and unhealthy, whereas her former children had been remarkable for robustness; and it eventually became idiotic.

The author brings forward from Pinel, Georget, and Esquirol, some information as to the leading causes of insanity, particularly as connected with vivid temporary emotions of the mind, and he states, on the authority of the North American Quarterly Review, that, on the night following the drawing of a very attractive lottery scheme in London, where many had risked their all, *fifty* suicides took place. This must surely be a most serious exaggeration; and we are surprised that the author should be satisfied with stating so vaguely, an unlikely circumstance of this kind, which, as occurring in the metropolis, so readily admitted of confirmation or proof. The sex, season, mental constitution, and profession, afford little well marked subject of difference; but derangement is most peculiarly attendant on civilization, and very little known in the barbarous or less refined countries. He gives many examples of great men, and particularly poets, who have become deranged or much influenced by mental delusions, or who have been much subject to hypochondriasis; and states, but without any evidence which has come to our knowledge, that Sir Walter Scott had suffered under the latter form of disease. The whole of Sir Walter Scott's life, on the contrary, shows a remarkable and never-ceasing cheerfulness and good temper; a disposition at all times to look at the bright side of things, great command of the attention and the powers of the mind, and most rational control over all the emotions and affections under the most adverse and unfavourable circumstances.

The fourth chapter, that on the *proximate causes* of insanity, introduces some observations on the important connection which exists between the sensorium, as well with the power of perception or sensation, as with the proper exercise of the intellectual faculties. The author considers all derangement of mind as necessarily connected with a diseased condition of the brain, and, as we may be led to infer from what we have already said, in respect to his opinions on this point, he concludes, that "when one faculty of the mind is deranged, the organ which is the medium for the manifestation of this faculty, is in a state of disease." He feels it difficult to conceive, that mental derangement, under all the variety of circumstances in which it appears, is "one and the same disease; but still he attributes the whole of the phenomena, by which it may at any time be characterized, to organic changes, modified by the general constitution of the patient. Undue vascular or inflammatory action, he regards as the general precursor of organic changes, and states, as he tells us, on the authority of Dr Elliotson, and several other physicians, that "when any particular organ of the brain is in a state of disease," "an excess of heat may be observed by the application of the hand to the seat of the affected organ."

That inflammation of any part of the substance of the brain, may be able to communicate the increased heat with which it is attended, to the external surface of the head, is not unnatural; but that the particular organ supposed by phrenologists to be the seat of mental malady, having no set of vessels peculiar to itself, should have its undue vascular or inflammatory action completely insulated, so as to be incapable of diffusion to contiguous parts of the brain, is quite at variance with correct pathology; but if it were not so, the meninges of the brain, not themselves necessarily implicated in the disease, must be the means by which the increased temperature is conveyed externally. The meninges must transmit it to the bony structure; this to the pericranium; and the pericranium to the scalp; so that there is a regular process of diffusion, which must entirely prevent any legitimate inference from being deducible as to the state of a very small and limited portion of the substance of the brain, over which it lies, and from which it is so completely separated. If all these consecutive portions formed one whole, and constituted one organ, the idea of an increased and defined temperature, might, to a certain extent, be admissible; but as a fair indication of what is passing within, and as a philosophical mode of coming to any correct conclusion on so difficult a subject, we must view the phenomenon contended for as entirely delusive.

The *general and exciting causes of insanity* come next under the cognizance of the author in his fifth chapter.

In idiocy, congenital malformation of the brain is the most frequent exciting cause; and this, the author thinks, is to be inferred, when the usual external signs of it are wanting. Idiots may sometimes have unusual endowments, in certain respects; and if any time among the sane, there is any faculty wanting, as that connected with the appreciation of colours or musical sounds, such persons, the author thinks, are to be regarded as idiots, to all intents and purposes, as far as tones and colours are concerned.

Mechanical and debilitating causes may give rise to insanity, as likewise may various morbid actions in distant parts of the body, particularly the intestinal canal. The author denies the existence of moral causes of insanity; but is of opinion, that, if "the organ of any one of the propensities, sentiments, or intellectual powers" is "strongly excited, or unpleasantly affected," it "is liable to become deranged in its functions:" and this may, by sympathy, be propagated to neighbouring organs, so as speedily to convert "the temperate and judicious individual" to the "raving and incoherent maniac." If love, joy, grief, anger, anxieties of nervous kinds, separation from friends, changes of fortune, or religious enthusiasm, be the apparent causes of insanity, they are only so according to the author inasmuch as the central organs, without which those particular feelings could have no existence, are more or less affected.

It would appear, however, that religious enthusiasm is much seldomer the existing cause of insanity, than is usually imagined; and that the impressions of a religious character which occasionally exhibit themselves in the insane, are usually superadded to, and independent of the original cause.

The sixth chapter is on the *forms of Insanity*, and on *its duration*, and *effects on mortality*.

Mental derangement is occasionally sudden in its attacks; but sometimes it comes on very gradually, and with those appearances of moral revolutions "which are often neglected, or fatally mistaken." The author makes some observations on the divisions of insanity, characterized by the designations of *dementia*, *monomania*, *melancholia*, and *mania*; and considers it of much importance to distinguish accurately between the intermission of an attack which is only an abatement of intensity, and a lucid interval, when there is a proper suspension of the disease. On the duration of attacks, and the proportion of recoveries, Mr Neville quotes the authority of various documents, to some of which we have already had occasion to advert in previous articles.

Chapter seventh is on the *Morbid Anatomy of the Brain* in insanity.

In this chapter, the author adverts to the statements of various pathologists, in respect to diseased appearances; but as far as we can observe, he has had few opportunities of personal observation on the subject. Pinel discovered in the brains of the insane nothing which could account for their malady; and Esquirol, who originally agreed with Pinel in his sentiments, has lately seen reason to admit the frequent existence of organic disease in the brains of the deranged. But then he is of opinion that such organic lesion has no necessary connection with the disease, inasmuch as analogous alterations have been met with, in the bodies of those who died unaffected with insanity. This species of reasoning, however, has no weight with the author, who informs us that we are never to lose sight of the fact, that insanity "is in most instances no more than a functional malady," that is, a disease in which symptoms arise from a change in the mode of action of the organs, depending on alterations "so slight, as to be almost or altogether imperceptible after death." To doubt the logical accuracy of the author's reasoning, would, he thinks, be as unreasonable as to question the existence of functional derangement in the organs of circulation and digestion during life, because the heart and stomach were found to be unaffected with any evident organic derangement after death. But the author overlooks an important difference between the cases; for while the connection which circulation and digestion have with the heart and stomach, is a matter of universal admission, it is yet to be proved that there are such things as organs of self-approbation, of religious sentiments, of secretiveness, or of any of the numerous divisions into which every portion of the superior surface of the encephalon, or perhaps more properly speaking, its whole substance, is so minutely divided. It is not necessary for those who may be so unorthodox in reference to the creeds of the phrenologists, as to entertain doubts respecting the existence in the brain of the various organs which are contended for, to repel one hypothesis, by the manufacture of another. "Let it only be conceived," says the author, "that the brain is the seat of the affections and intellectual faculties, and there is no difficulty about the matter." It must then be admitted, that "insanity is not an entity superadded to the organisation, but evidence of derangement in the organisation itself, intrusted with the manifestation of the faculties or functions deranged." This is, however, taking the whole question for granted; it is the assumption of a postulate of somewhat too bold and unreasonable a character; and though it is impossible to deny the usual attributes of the sensorium, yet it is no derogation to the physician and the philosopher, to confess an entire ignorance as to the hitherto inscrutable connection which exists between mind and body, or to suppose with the author, and the lamented and amiable Spurzheim, in whose wake he steadily follows, that nothing short of an implicit adhesion to their principles, is to afford any thing like a sound and philosophical basis for the treatment of mental alienation.

The admission of frequent morbid changes in the brain, in cases of insanity, does not necessarily throw us into phrenology; nor does it at all follow, that we must be prepared to identify organs with faculties, if we even admit that frequent injection of the capillaries of the encephalic vessels and its membranes, which, with some other morbid appearances, the author considers as continual accompaniments of disturbance of faculties of the mind. Neither does the phrenological division of faculties much affect practice, unless, indeed, the curative means which are adopted could be confined to the minute segments in which the affected parts are situated, which it is not pretended can be the case. General and local bleedings, local applications to the head, and the various descriptions of physical agents which are employed in the treatment of insanity, can all of them be used, and are, in fact, all of them used with as much success, and as much discrimination, without the concession of a single organic faculty, as with the complete admission of the whole series.

The eighth chapter relates to the *prognosis* in functional derangement of the brain.

Recovery is more difficult when there is hereditary disposition to the disease, than when this is wanting; and the prognosis is always more favourable in sympathetic mental derangement, as puerperal mania for example, or that from the use of spirituous liquors, than in the idiopathic form. Mania is more generally cured than monomania, or melancholia, or when mania and melancholia alternate with each other. The prognosis is better with robust, than delicate individuals; in middle age than with the young or old; and upon the whole with females than males.

The last two chapters, namely, the ninth and tenth, are employed in considering, the one, the *treatment*, and the other, the *medical treatment, of functional derangement of the brain;* though there seems no adequate reason for more than one chapter being occupied on the subject. In the former of these two the author follows Dr A. Combe in the means recommended by him as the most proper to abate predisposition, and to ward off impending or incipient functional derangement of the brain. They consist principally in the regulation of occupation and exercise, particularly with the young; and in various judicious modes of moral management.

The curative means which are regarded as best adapted for the treatment of insanity, to which the tenth chapter is appropriated, are ushered in by observations on the importance of medical agencies in reference to the subject. But these, it must be remarked, whether well or ill employed, have been in use long before either Gall or Spurzheim were in being; and the quotations which the author gives us from Dr Burrows, the Retreat near York, and the Connecticut Retreat in North America, (in all which the cures in recent cases amounted to from 88 to 91 per cent.) show decidedly, that an adoption of the doctrines of phrenology is not necessary for conducting, with ability and success, the medical treatment of the insane.

There seems to be nothing new in the practice recommended by the author; but the means on which he, and the most judicious practitioners in insanity mainly depend, namely, local and general blood-letting, the warm-bath, the application of cold to the head, and the use of purgatives, are pointed out, under the various modifications which are necessary in their employment, with judgment and discrimination. Of blisters to the scalp, he disapproves; but he has seen much advantage from the use of moxa, and the ointment of tartarized antimony, and hydriodate of potash. Keeping up the strength by the judicious use of tonics, and of such means as are best adapted to promote general health, he considers as of much importance; and states the great advantage which he has derived, when the action of the digestive organs has been defective, as indicated by the production of acidity, in the use of carbonate of soda to the extent of one, two, three, or even four drachms at a time. The power of considerable doses of this medicine, in keeping the body in health, when nourishing food and little exercise were used, was evinced by experiments made on a couple of monkeys, and a couple of Newfoundland whelps. The animals, which had two drachms of carbonate of soda given them three times a-day with their food, were at the end of six months in perfect health; while the others were thin, spiritless, and diseased; and one of them, on being killed, showed much organic disease in the abdomen. The author gives two or three cases of dyspeptic persons, not insane, in whom the beneficial effects of carbonate of soda, in the quantity of from half an ounce to an ounce in the day, in divided doses, with a light bitter infusion, were strikingly manifested.

The proper regulation of temperature, which should be maintained at from 55° to 60° of Fahrenheit, the author considers to be of great importance; and in the early stages of the disease, he employs with advantage some of the salts of morphia to procure rest. Digitalis he has seen useful when there is unusual throbbing of the heart; but mercury generally does harm. Proper exercise out of doors, so as not to produce increased velocity of the circulation, or to take away appetite; and amusements within doors, which combine some degree of bodily exercise with the occupation which they afford to the mind, he considers as highly advantageous.

The author considers it as foreign to his purpose to enter at length on the moral treatment of mental derangement; and he therefore says very little on the subject. This is a circumstance to be regretted; for there would have been much interest in an exposition of the various modes in which the moral treatment of derangement is carried on at Earls Court. We had looked, indeed, to this, as the distinctive character of the present work, and as being likely to give it somewhat of an original aspect. When the province of inspection is confined to a small number of cases only, we had hoped, likewise, that the results would have been brought forward of careful observation on every particular to which the man of discernment directs his inquiries during his professional attendance. We see that another work by Mr Neville is in progress, and we shall be happy in finding that these deficits will be supplied in it from so good a field of practical observation as Earls Court affords.

ART. IV.—*The Elements of Materia Medica; comprehending the Natural History, Preparation, Properties, Composition, Effects, and Uses of Medicines.* Part I. *Containing the General Action and Classification of Medicines, and the Mineral Materia Medica.* By JONATHAN PEREIRA, F. R. S., L. S. &c. London, 1839. Pp. 559.

WITHIN the last twelve years, many works of great merit have appeared on the Materia Medica, notices of which were given in the forty-first volume of this Journal, (p. 471,) to which we refer our readers. The work of Mr Pereira, so far as we are able to judge from the volume before us, bids fair to rival its predecessors.

The greater portion of the information contained in this volume was published as Lectures, in the seventeenth volume of the London Medical Gazette; and the favourable manner in which these were received by the profession, have no doubt had their weight in inducing the author to publish his investigations in the present form. The plan on which the work is to be conducted, (for only the first part has as yet appeared,) accords well with the advanced state of this branch of science,—the author proposing to classify the organized matters according to their natural-historical relations; whilst the inorganic substances, treated of in the present volume, are arranged according to a chemical classification. We were the more pleased to observe this, from knowing that a similar mode of teaching the science has, for several years past, been successfully introduced into the schools, and more particularly into the University of Edinburgh, by the late Dr Andrew Duncan, Junior, and by his distinguished successor, the present Professor of Materia Medica.

In the volume before us, the author, after a definition of terms, treats the general subject of Therapeutics under the following heads.

2. *Means of ascertaining the operation of Medicines.*—The author discusses this subject under two heads; first, as to the physical and chemical properties of medicines; and secondly, as to the phenomena caused by their contact with the animal body. Under the first of these, the author notices the general connection found to exist in the vegetable kingdom, between similarity of form and their medicinal properties, and mentions a few of the natural families where this coincidence has been observed in all the species yet known. He also enumerates several of those families where very dissimilar properties exist. He remarks, that though the chemical properties may assist occasionally in determining the influence which medicinal agents may have over the organism, they

are not in general of much service, as bodies very dissimilar in chemical properties have frequently the same action.

3. *Mode of action of Medicines.*—This the author treats in two sections; the one where the action depends on the active force of the medicine, the other on the vital force of the organism. Their mode of action is mechanical, when their operation depends on their weight, cohesion, external form or motion; but the efficacy of few medicinal agents can be solely referred to this influence. It is chemical when they act by their mutual affinities. That some agents act in this manner it is impossible to deny, though to what extent seems to be difficult to determine, as when external agents are taken into the system they become subject to a superior power, and are no longer at liberty to obey the ordinary laws of chemical affinity. Medicines act dynamically when their action cannot be referred either to chemical or mechanical agencies.

4. *Physiological effects of Medicines.*—These are considered first as to their local or topical, and next as to their remote effects.

5. *Absorption of Medicines.*—The proofs of absorption are, the disappearance of a medicinal agent from a shut cavity, and its detection in other parts of the body, as in the blood, in the secretions, and in the solids. Mr Pereira details the circumstances which appear to establish that absorption takes place, through the agency of the veins. These are, 1. the detection of colouring, odorous, and saline substances in the venous blood; 2. Magendie's experiment; 3. the effects of poisons being produced though the lacteals be tied; 4. the fact that, when the blood-vessels are tied, poisons do not act; and 5. the rapidity of absorption. The mechanism of absorption is best explained, according to the author, by assuming the existence of two powers or agencies, the one physical, (Imbibition of Magendie, Exosmose and Endosmose of Dutrochet,) and the other vital. The question as to whether the absorption of a medicine or poison is essential to the production of its remote effects, is commented on with apparent impartiality, and the reasoning in favour of, and the objections to that theory, very fairly stated. The conclusion arrived at is, "that, in a large number of instances at least, if not in all, the operation of a medicine on remote parts of the system depends on its absorption. Medicines and poisons which have entered the blood appear to affect distant organs, first, by modifying or altering the properties of the blood, and thereby unfitting it for carrying on the functions of the body; secondly, by pervading the structure of the organ acted on; and thirdly, by acting on the lining membrane of the blood-vessels.

6. *Operation of Medicines by Nervous energy.*—The author, after noticing the proofs that some substances, as hydrocyanic acid, act independently of absorption, through the agency of the ner-

vous system, concludes, that "the operation of some medicines seems to be best explained by supposing the previous absorption of these agents; while that of other substances appears to be most satisfactorily accounted for by presuming they affect the nerves independently of absorption."

7. *Parts affected by the remote action of Medicines.*—These are the nervous, circulating, respiratory, and digestive systems, the secreting and exhaling organs, the lymphatic, muscular, and sexual systems, and the urinary organs.

8. *Of the Nature and quality of the actions induced by Medicines.*—As medicinal agents increase, diminish, or alter the vital actions, the author has arranged them in three classes, viz. stimulants; contra-stimulants or sedatives; and alteratives. He also notices shortly, and controverts the Brunonian theory, and the theory of contra-stimulus of the Italian school.

9. *Circumstances which modify the effects of Medicines.*—These the author considers under two heads; those relating to the medicine, and those relating to the organism. The last of these heads is treated at considerable length, and many interesting facts are brought together, showing the modifying effects of age, sex, habit, disease, climate, race, idiosyncrasy, &c.

8. (? 10.) *Therapeutical Effects of Medicines.*—Therapeutical effects are produced in two ways; first, by the influence of a medicine over the causes of a disease; and, secondly, by modifying the actions of one or more parts of the system. The fundamental methods of cure, of which there are three, and which have been denominated the antipathic, homoeopathic, and allopathic, are commented on with ability and seeming impartiality.

9. *Parts to which Medicines are applied.*—These are treated by the author in the following order; 1. Applications to the skin. 2. Applications to the mucous membranes. In speaking of applications to the vagino-uterine membrane, he mentions a singular fact with regard to the power of carbonic acid gas in allaying uterine irritation. 3. Applications to the serous membranes. 4. Applications to ulcers, wounds, and abscesses. 5. Injection of medicines into the veins. He then states shortly the agency of galvanism in introducing certain medicinal particles into the blood; but he mentions that he has not been able, in his own experience, to verify the results to which M. Fabré-Palaprat had arrived, with regard to the transmission of iodine through the body by its agency. He adds, "it is not improbable, however, that electricity may promote absorption, either by increasing endosmosis, or by acting as a stimulus to the blood-vessels and lymphatics."

10. *Classification of Medicines.*—This section contains valuable lists of the works on the Materia Medica, arranged according to the different classifications which have been adopted in the se-

veral works. These are the alphabetical arrangement; that founded on the sensible qualities of medicines, as colour, taste, and odour; the classification founded on the natural-historical properties; that founded on the chemical constituents; and that founded on the physiological effects of medicines. He likewise takes notice of those who have arranged medicinal agents according to the Brunonian doctrine; according to the new medical doctrine of Broussais; and according to the particular structure or organ they affect.

11. *Physiological classes of Medicines.*—The author's classification seems to be founded on those of Eberle and Vogt, but appears to be an improvement on both. It is probably as perfect as in the present state of the science can be expected. His classes are eleven in number; 1. Cerebro-spinants; 2. Stimulants, Incitants, or Excitants; 3. Tonics; 4. Emollients; 5. Refrigerants or Temperants; 6. Evacuants; 7. Abortiva seu Acceleratores Partus; 8. Caustics; 9. Rubefacients, Vesicants, and Suppurants; 10. Acids; 11. Alkalies.

The first of these, the cerebro-spinants, Mr Pereira divides into ten groups. We are inclined to question the utility of this excessive minuteness of subdivision, for we find not only those medicinal agents whose primary action is on the cerebro-spinal system, but many others whose primary action is on the vascular, muscular, and other systems, placed under this head by the author.

The second class, or stimulants, incitants or excitants, is divided into seven groups. The first group consists of the volatile pungent stimuli; the second and third are mere varieties of one another, the one consisting of the aromatic or savoury herbs, and of the carminative seeds of the *Umbelliferæ;* and the other of the substances called spices. The fourth group includes the solid resins, the oleo-resins, the balsams, and the fetid gum-resins. The fifth group is formed to include ammonia and its salts, the empyreumatic oils, phosphorus, musk and castor; the volatile nervines as they have been called. The sixth group includes camphor, valerian, oil of cajuputi, &c. the volatile excitants or ætherio-oleosa of Vogt. The seventh group includes the spirituosa or inebriants, as alcohol, wine, ether, &c.

The third class, the Tonics, are divided into six groups, which, however, seem more natural divisions. They consist of, 1. Vegetable tonics which possess bitterness with little or no astringency, as quassia, gentian, &c. 2. Those vegetable tonics which possess considerable astringency with little or no bitterness, as nut-galls, catechu, rhatany, &c.; the astringents of authors. 3. Vegetable tonics which possess bitterness and astringency in an eminent degree, as cinchona bark, elm bark, willow bark. 4. Vegetable tonics which possess bitterness, with an aromatic flavour, derived from the presence of volatile oil, and in some cases with astringency likewise. This group contains cascarilla, Angustura bark, hops, &c. 5. The acid tonics, including the mineral acids, and perhaps also alum. 6. The metallic tonics, of which the chief are the preparations of iron.

The fourth class consists of the emollients, and the fifth class of the refrigerants, of which it is unnecessary to take notice.

The sixth class, or evacuants, contains perhaps the most useful remedies in the Materia Medica, and are treated by the author at considerable length under the heads of 1. Diaphoretics or sudorifics; 2. Errhines; 3. Sialogogues; 4. Expectorants; 5. Emetics; 6. Cathartics or purgatives; 7. Chologogues; 8. Diuretics; and 9. Emmenagogues. Valuable observations are given on each of these classes.

The remaining classes are only shortly noticed by the author.

Here ends the general or introductory part of this volume. The remainder is taken up with the particular detail of the articles of the Materia Medica belonging to the inorganic kingdom. As we stated before, they are arranged according to a chemical classification. The fullest details are given of each substance under the heads of History, Synonymes, Etymology, Natural History, Preparation, Properties, Characteristics, Physiological Effects on Animals and on Man, Morbid Appearances, Modus Operandi, Uses, Dose, and Antidotes.

It would be quite impossible to give anything like an analysis of the information contained in this part of the volume. We have merely to notice, that, so far as we have observed, it contains all the recently introduced medicines belonging to the inorganic class. Of these, Creosote, and the preparations of iodine, are those which have attracted the chief attention in the medical world, and minute accounts of these medicinal agents are to be found in this volume.

Under the article *Aqua*, water, is to be found a very comprehensive account of the internal and external uses of that fluid. This is followed by short notices of the mineral waters.

In treating of arsenic, the author does not seem to be aware of the powerful action of arsenical preparations over determinations of blood to the head. Whether this depends on the specific influence of the arsenic over the vascular or nervous systems, seems difficult to determine, yet that it does possess such a power seems to be beyond a doubt.

Fifty-seven pages are devoted to the consideration of mercury, and its preparations; and the article includes every thing known with regard to this most important agent.

In speaking of the physiological effects of the iodide of iron, one of the substances most recently introduced, he says, " in small and repeated doses its effects are not very obvious, save that of blackening the stools;" and again, "when it does not purge, it frequently acts as a diuretic." We have had several opportunities of trying the effects of this medicine in various diseases, prepared according to the directions of Dr A. T. Thomson, and find that, in a very great number of the cases in which its beneficial or therapeutic effects were most strongly marked, it increased in a remarkable degree the cutaneous exhalation. We have found it of especial service in chronic skin diseases, in secondary syphilis, and in chronic rheumatism; but have rarely seen any beneficial results from its use in chlorosis or amenorrhœa, or disturbance of the uterine functions. We have never found any good to result from carrying the dose to the largest which the stomach would bear, but rather the reverse; for in most of the cases where a beneficial result has followed its use, the dose has been small, but very frequently repeated. As a tonic, it appears to be far inferior to the sulphate or the tincture of the muriate of iron, which in atonic diseases, and especially in uterine irregularities, appear to be almost specifics. It appears to be a fact not generally known, that the combination of the sulphate of iron with aloes, so increases the cathartic effect of the aloes, that two or three grains of the latter with half a grain of the former will produce effects equal to ten or twelve grains of the aloes uncombined.

While we give the introducers and discoverers of new medicines every credit for their zeal in the promotion of the healing art, we are far from believing that the profession is benefited by the copious additions which are every day making to the lists of the Materia Medica. These have a great tendency to distract the attention from those well-known powerful therapeutic agents already in use. We have only to look at the work before us to be perfectly satisfied, that at least one-half of the substances might be cut off without losing a single remedy of the least importance. If the already overburdened lists go on increasing as they have done, a work on the Materia Medica will be at the same time a system of Chemistry and of Botany. In fact, instead of endeavouring by all means to increase the number of medicinal agents, we believe that the public as well as the profession would equally be gainers by a judicious selection from the existing lists, in which all those articles, the administration of which is questionable or positively useless, should be rejected.

The volume is illustrated with numerous wood-cuts, and has, besides, diagrams of the decomposition and re-action of the different chemical agents. It exhibits throughout great research, and an intimate knowledge of the subject.

ART. V.—*Etudes Historiques et Critiques sur la Vie et la Doctrine d'Hippocrate, et sur l'etat de la Médecine avant lui.* Par M.-S. HOUDART, Docteur en Medecine de la Faculté de Paris. 8vo. Pp. 320. Notes, pp. 125. Paris, 1836. *Historical and Critical Inquiries on the Life and Doctrines of Hippocrates, and on the state of Medicine before his time.* By M.-S. HOUDART.

THIS work has appeared under circumstances calculated to excite interest and sympathy. The subject of it was suggested by an accidental conversation with a friend, in the Vale of Montmorency many years since. It gave rise to an inaugural dissertation which the author was preparing to publish under an extended form, ten years ago, when he was attacked with a fit of apoplexy, from which he with difficulty recovered. During his convalescence he resumed his labours, and has at length, with creditable industry, been able to bring forward the present volume, notwithstanding the disadvantages under which he laboured, of impaired health, and difficult access to works of reference.

The work is divided into three parts. In the first, the author treats of what is known of *Hippocrates, personally*; in the second, he examines *the state of medicine previous to his time*; and, in the third, gives an account of the *doctrines of Hippocrates*.

As the two first divisions afford some view of discussions relative to the ancient history of medicine, which may not be familiar to many of our readers, we shall principally confine ourselves, in the present article, to an examination of these parts of the work before us.

With regard to the *personal history* of Hippocrates, it is not to be supposed that the author adds much to the variety of speculations, which have been entertained upon this subject. He does not throw any doubts on his existence, though this was done in an inaugural dissertation published some years since at Paris, to the no small annoyance of the medical dignitaries of the day, who were outraged by so great a heresy; but he follows those authors who are of opinion, that very great obscurity hangs over every thing which relates to him.

It is certainly a remarkable circumstance, that Hippocrates should have been so little mentioned, either by contemporaries, or by the principal historians, for a long period after his time. Plato and Aristotle give the slightest possible notice of him, and the same may be said of Herodotus and Xenophon, whose scanty mention rather, indeed, applies to some other person of the same name, than the divine old man himself. But it is most of all singular, that Thucydides, though he treats at great length and with much precision, of the plague of Athens, says nothing of Hippocrates; from which the author would infer, "either that Hippocrates, it he were at Athens at the time of the plague, enjoyed no gre lebrity, or that he was of the number of those, of whom Tl dides spoke but little favourably, and who were regarded by as knowing nothing of the complaint."

The information which has been handed to us in respe Hippocrates has been derived, almost entirely, from some ments left by Soranus, who was a physician living in the tin Adrian and Trajan, in the latter part of the first and begin of the second century. This is, therefore, nearly 600 years the period of Hippocrates, who was born about 460 years b the Christian era; and hence the author is of opinion, that a many inaccuracies may naturally be supposed to exist in the s ments relative to him, from the want of satisfactory docum One of the most material facts related of Hippocrates is, th possessed great celebrity in Greece, and was called by Perdi King of Macedon, conjointly with Euriphon, from Cnidus, to him of a slow fever, which was adroitly discovered by Hippoc to arise from an attachment to Phila, the mistress of his dece father. This particular incident is, however, not altogether void of suspicion, since a similar fact is stated of Erasistrat the Court of Seleucus Nicanor; and Euriphon was found to existed some time before Hippocrates, and could not, there be the companion of his visit to Perdiccas. On comparing Phenomena of the Plague of Athens, as described by Thucyd with some of the cases mentioned in the Epidemics of Hi crates, the author considers that there is no resemblance; a of opinion, that, though the latter have been thought by son having occurred at Athens, they really appeared at Thasus island in the Grecian Archipelago. Athens, it is stated, is n once mentioned by Hippocrates, as having been the scene o practice or observations, though he mentions Thasus, La Abdera, Platamon, and other places; and hence the author cludes, that the idea of his having practised at Athens at all, still more at the time of the plague, is altogether erroneous. confirmation of this inference, the authority of Galen is ment ed, who states, that his having seen five cases of a particular plaint, of which Hippocrates had not seen one, might be acco ed for from his having occupied so much larger a field of pra than the latter, one quarter of Rome being larger than any in which Hippocrates had ever lived. On the suppositio Hippocrates having practised at Athens, which is represente have contained 180,000 inhabitants, this would have give Rome, with its fourteen quarters, the incredible populatio above 2,500,000.

Upon the refusal of Hippocrates to accept the invitatio Artaxerxes Longimanus to visit Persia during the attack of pla though stated with much confidence, even so far back as the t of Cato the Censor, much doubt has been thrown by many writers of authority, but particularly Freret, Haller, and Schulze; and the whole history of the interview between Hippocrates and Democritus, in the Island of Abdera, is regarded by Schulze, and the celebrated critic Scaliger, as exceedingly apocryphal. The latter, indeed, admits, that the letters on which the evidence in relation to the call which Hippocrates received to the Island of Abdera are ancient; but he states, that fabrications were familiar to the Greeks, and that he considers the whole correspondence as an interpolation. Bayle, in his Dictionary, examines at length the whole of the interviews which were stated to have taken place between Hippocrates and Democritus, and comes to the same conclusion. The present of ten talents, which it has been stated that the inhabitants of Abdera made to Hippocrates for his visit, is considered as absurdly large for a poor place such as that, where Plutarch informs us, that the whole treasure of which Philip of Macedon was possessed for defraying the expenses of the war in which he was engaged did not amount to more than seventy talents.

The severity of historical investigation, affords some degree of mortification to the lovers of agreeable anecdotes; but this must be submitted to, in an inquiry after truth; and it is perhaps creditable to the good temper and good feeling of narrators, that they are so often disposed to adorn their heroes with a larger portion of good qualities than may fairly belong to them. Even the virtues of Hippocrates do not escape the Argus eyes of the philologist; for some of those parts of his works, which depict most strongly the benevolence, candour, and rectitude of his character, have not been acknowledged as genuine.

In reducing the literary labours, and the professional merits of Hippocrates to their just dimensions, M. Houdart follows closely the steps of Sprengel, and employs much the same facts and argumentative considerations which have been adduced by that learned author.

The difference which exists in the style of many of the works attributed to Hippocrates, as well the want of accordance and consistency in some of the opinions referred to him, afford a reasonable ground for suspecting a considerable portion of that species of interpolation to which the works of the ancients were in many respects peculiarly liable. The formation of large libraries was an object of high emulation among the great; and as no expense was spared in effecting the object, and this was in proportion to the reputation of the author, to the lovers of gain, a great inducement was afforded to increase the number of works attributed to names, which were advantageously known in literature. This kind of speculation was too lucrative, and the name of Hippocrates too celebrated to make it likely that he would be spared; and to this cause, the author attributes the number of treatises which now

present themselves in his works, and of which he cannot be reg
ed as the author. These literary speculations were carried to s
an extent, that the death of several distinguished authors
not waited for, before supposititious works were attributed to th
and Galen assures us, that there were publicly exposed to s
under his own name, complete treatises to which he had never co
buted the smallest portion. It was supposed, too, that, in co
quence of many of the writings of Hippocrates having been
stroyed at the fire of Alexandria, spurious productions were
stituted for them; and that in ancient times, likewise, manusc
were often concealed or burnt, either through envy, or the de
of assuming the credit of authorship, without risk of detect
The sons and immediate successors of Hippocrates, likew
Thessalus and Draco, and Polybus, his son-in-law, having ad
ed different principles from those of their august predecessor, m
numerous interpolations in his works, for the purpose of reco
ing them to the new doctrines embraced by them. Under all t
circumstances, the author is of opinion, that of more than ei
treatises which are attributed to him, the eight following only
genuine; and that even the last of these, the *oath*, has been
garded by many as standing on somewhat equivocal ground;
Galen does not notice it, while Sprengel doubts its authenti
1*st*, The Aphorisms; 2*d*, The Prognostics; 3*d*, The 1st
3d books of Epidemics; 4*th*, The treatise on Regimen in A
Diseases; 5*th*, That on wounds of the Head; 6*th*, On Air, W
and Places; 7*th*, The Workshop of the Physician; 8*th*,
Oath.

The *second part* of M. Houdart's work is on the *state of*
dicine previous to the *time of Hippocrates.*

Notwithstanding the slow growth of science, and the labor
accumulation of facts, of which it is composed, Hippocrates is
nerally viewed as the father, the generator of medical knowle
before whose time everything relating to the healing art was in
most grievous state of rudeness and barbarism. It would
pear, indeed, as the author states, that medicine had issued f
his brain, just as perfectly accomplished and accoutred, as
nerva from the brain of Jupiter; but yet, considering that the
cessities of the world must have given rise to a demand for
exercise of the healing art, at the earliest period of society,
that, at the time of Hippocrates, great advances had been m
in every department of the arts and sciences, it could hardly
imagined that there was not a great foundation of medical kn
ledge existing in his time. Some of the most noble cities of
tiquity had been built, which long remained distinguished ex
ples of civil and military architecture; laws had been made; c
merce, navigation, and the arts cultivated; the year regulat
the fixed points of the season marked out; and, in short, soc

had been framed, with all its dependencies, for individual and general benefit, as well in matters required for ordinary comforts, as those which concern taste and luxury; and hence, the author infers, that it would have been strange, if so necessary an art as medicine is, had, under all these circumstances, been the only one which possessed no existence, or had made no advancement at that period.

But Heraclitus, a contemporary of Hippocrates, has been regarded as the first Greek author who wrote in prose; and from this it has been inferred, that no works of science could have existed previous to this period, as philosophers would not write in verse. The obscurity of Heraclitus's writings, however, was complained of in a letter written to him by Darius, the son of Hydaspes, who stated that they could not be comprehended "by those who had gone through the greatest number of volumes;" which shows that at that time volumes were accessible; and Freret was of opinion, that prose, as mentioned by Pliny, applied alone to a certain harmonious and elegant structure of sentences, which this mode of writing only assumed, long after its first and original employment. The absence of medical practitioners in Rome for 600 years after its foundation is considered by the author as an unlikely, if not an extravagant supposition; for it is stated by Dionysius of Halicarnassus, that there were physicians in the kingdom or city of Alba close by Rome; and that 300 years after the building of the city, during the consulship of Publius Horatius and Sextus Quintilius, a contagious disease made its appearance at Rome, which there were not sufficient medical men to treat. In the life, too, of Cato the Censor, Plutarch speaks of a Roman Ambassador sent to the Bithynians, who had a large breach on his head, from having undergone the operation of the trepan, which could not have been done without much professional skill. But much of the literature and science of the Greeks was derived from Egypt, in which there is every reason to conclude that physicians and medical writings existed from a very early period.

Many physicians were mentioned by name by several of the ancient writers; and the Ptolemies, who were the great founders of the Alexandrian Library, were precise in their directions, to collect the works of the most eminent physicians from all parts of the world, which Demetrius of Phalera carried into ample effect. The collection of writings ascribed to Hermes or Mercury, and known under the name of Βιϐλοι Ἑρμετικοι, (*libri Hermetici,*) are stated to have contained six volumes of it, two on subjects of medicine, and a second volume is mentioned by Diodorus Siculus as being the composition of an union of ancient physicians. But it must be observed, that all the best authorities, from Jamblicus and Galen in ancient times, to Marsham and Sprengel in modern days, regarded these works as altogether apocryphal, and the fabrication of the monks of Alexandria or some later or contemporary sophists.

The various sources of information which Egypt opened to the Greeks could not fail to supply Hippocrates, the author thinks, with much valuable professional knowledge; to which must be added, that medicine had been for many ages the hereditary study of his family. But if more direct evidence is required of the high state of advancement which medicine attained at his time, this is afforded by the *Coacæ Prænotiones*, which are published with the works of Hippocrates, and which are found to be the basis of much of the most important and sterling information which is contained in the most valuable of his works. The author places side by side, various extracts from the *Coacæ Prænotiones*, and the Prognostics of Hippocrates, from which the striking accordance between many parts of those two works are distinctly seen. But then it is to be observed with regard to this, which he considers so irrefragable an argument on the subject, that it goes on the supposition, that the *Coacæ Prænotiones* were in actual existence previous to the time of Hippocrates; a point which is not altogether proved, though exceedingly probable.

Though the author admits the high merits of Hippocrates, he does not consider his claims to be regarded as the father of physic, to stand on any better ground than that of Homer, to be viewed as the father of poetry, or Newton, of philosophy. Cicero has no doubt of there having been poets long before the time of Homer, because nothing, he is of opinion, can be perfected at the immediate period of its invention; while every one knows what important advances in physical science had preceded the age of Newton.

The *third part* of the author's treatise, that which he devotes to a consideration *of the doctrines of Hippocrates*, does not call for much of our attention. M. Houdart has seen but little of disease, and its effects; and his criticisms on the opinions and practice of Hippocrates, though sometimes sufficiently appropriate, have no claim either to originality of view or nicety of discrimination. He is evidently under the constant and commanding influence of a system, which, like many of its ephemeral predecessors, is grateful to the natural indolence of the human mind, by promising an easy road to knowledge, and by reconciling, or appearing to reconcile, which are often unfortunately interchangeable terms, a great variety of facts to one comprehensive, though gratuitous principle. Broussais is the god of the worthy author's idolatry, and is worshipped by him as a mighty discoverer. M. Houdart professes a modest and compassionate unwillingness to despoil Hippocrates of his crown of immortality; but he has no compunction in employing a third person in the office. This is no less a personage than Justice herself, whom he occupies, in an imaginary frontispiece, with much apparent gratification on his part, at the waggery of the thought, in stripping the bust of Hippocrates of the wreath with which it had been so long unworthily decorated, and placing it on the bust of Broussais, under the inscription of " render unto Cæsar the things that are Cæsar's."

The varied, though irregular inquiries of above 2000 years, seem to have done nothing in settling and adjusting the precise claims of Hippocrates to public regard; and it was reserved for the great Broussais, " the illustrious founder of the Physiological School," according to the admiring and self-complacent author, " to break to pieces the sceptre of Hippocratism. Eternal thanks are due to his active and powerful genius, for the movement which he has known how to produce, on the somnolent spirits of his age; a movement which, by putting everything to the proof, has stirred up science to its foundations, and has given rise to that multiplicity of discoveries which have appeared around us. Oh, Broussais," continues this weak, but well meaning disciple, " thou hast acquired an imperishable glory; and already does thy name resound over the whole civilized world. If thy contemporaries do not render thee all that justice to which thou art entitled, posterity will avenge thy wrongs, by decorating thy forehead with the immortal palm, which is so well due to this title of benefactor of the human race." After this eulogistic rhapsody, the author gives a short statement of some of the principal doctrines of Hippocrates; and then goes on to a long and tedious exposition of views, often crude and indistinct, and never original, on medicine, the animal economy, physics, and physical causes.

His lucubrations have conducted him to downright materialism, " Functions and vital principles have no real existence; and with them, therefore, the physician need not trouble himself. He is solely concerned with organs, and their different mode of lesion," the whole phenomena of life depending on the system of healthy and deranged organization, are interruptive of the chain which produces death. The author views the existence of a distinct vital principle as a perfect chimera; and seems to repose with complete satisfaction and confidence on such reasoning as the following;—" If life were a gift of Providence, it must follow that our globe must be of divine origin; the one appears to me the necessary consequence of the other. But the opinion of the greatest geologists of the present day is in no way favourable to such an idea; they admit, on the other hand, that the earth, which is at present a solid and compact mass, was, in its primitive state, under the same form which the nebulæ, that the telescope of Herschel has laid open to us, exhibit in the heavens." And this farrago, the author would attempt to pass off for induction and philosophy.

ART. VI.—*The Life of* EDWARD JENNER, M. D., LL
F. R. S., Physician Extraordinary to His Majesty George
Foreign Associate of the National Institute of France, &c.
with Illustrations of his Doctrines, and Selections from his
respondence. By JOHN BARON, M. D., F. R. S., Late
nior Physician to the General Infirmary, and Consulting P
sician to the Lunatic Asylum at Gloucester, Fellow of
Royal Medical and Chirurgical Society, &c. &c. 2 vols.
Lond. 1838.

THE origin and progress of vaccination forms one of the
important and interesting parts of the history of medical scie
The discovery of an influence capable of operating in such a
on the constitution, as either to prevent or disarm of its terrors,
of the severest scourges of the human race, could not but be rec
ed with admiration and gratitude. But that this influence shoul
possessed by a disease, and one differing essentially in all its
prominent characters from that which it was intended to su
sede, went beyond the bounds of ordinary credence. The
nouncement of such a fact burst unexpectedly on the world, w
out the support of any point of previous analogy; but the evid
on which it depended was so judiciously and unostentatiously
tured, and the deductions so correctly made, as to afford to
illustrious discoverer of vaccination a full confidence of its re
acceptance.

In this he was not disappointed; and it was in a high de
honourable to the philanthropy, candour, and good feeling of
medical profession in every part of the world, that its practit
ers united so speedily in adopting vaccination, and in depreca
any farther employment of variolous inoculation, as tending to
up and diffuse a disastrous and loathsome disease.

The progress of vaccination was, however, much facilitated
the high character which Dr Jenner enjoyed for ability and ju
ment, and still more for single-mindedness and candour. He
sessed, likewise, in an eminent degree, suavity of manners;
open and ingenuous mind depicted strongly upon his countenan
an imperturbable good temper, depending much on the cons
and habitual cultivation and exercise of feelings of benevole
But when to all these were added, energy of mind, clearness of
tellect, and a natural and unpretending eloquence in his ordin
conversation, we cannot but admit how eminently he was qua
ed to give effect to his own views, and to be the expositor and
vocate of his own opinions.

It is therefore with peculiar pleasure that we take up the
sent work, which comes forward under the most favourable

pieces, as the production of a gentleman highly estimated in the profession, and well qualified, both by his talents, and his close friendship with Dr Jenner, to do justice to the character and opinions of that very distinguished person.

The position of vaccination has of late assumed a very serious aspect, and has created in the minds of its most strenuous supporters, much alarm and anxiety. When an attack of small-pox after vaccination was rare, and the symptoms mild, there was consolation in the leniency of the complaint, and in the consideration, that small-pox itself did not always defend the constitution against a second and even a severe attack of the same complaint. But the frequency, severity, and even occasional fatality of such occurrences do not admit of being reconciled to any known or well established principles of morbid action; and it is incumbent, therefore, on the profession generally, and, in particular, on those members of it, in this and other countries, whose opportunities of observation are the most extensive, to give the subject their most serious and attentive consideration. We shall keep it most anxiously in view; but do not feel it necessary, in the present article, to do more than express our deep conviction of its importance. There does not, indeed, appear to have been yet sufficient evidence obtained on which to form any satisfactory conclusions, either as to the extent to which our dependence on the protective influence of cow-pox is to be affected by the late failures; or the circumstances which govern that influence on the animal economy.

Dr Jenner was born on the 17th of May 1749, at the vicarage of Berkeley, in Gloucestershire, and was the third son of the Rev. Stephen Jenner, Rector of Rockhampton, and Vicar of Berkeley, by a daughter of the Rev. Henry Head, who once held the living of Berkeley. He lost his father at an early age, and was principally brought up by his eldest brother, the Rev. Stephen Jenner, Rector of Fittleton in Wiltshire. When about eight years of age, Dr Jenner was put to school at Wotton-under-Edge, with the Rev. Mr Clissold; and was next placed under the tuition of the Rev. Dr Washbourn, at Cirencester, with whom he continued till he became the pupil of Mr Ludlow, an eminent surgeon of Sodbury, near Bristol. From a very early period he was much attached to the study of natural history; was zealous in searching for fossils in the oolitic formation of the neighbourhood; and before he was nine years of age had made a collection of the nests of the dormouse. When he left Mr Ludlow, in the twenty-first year of his age, he went to London, to prosecute his professional studies under John Hunter, in whose family he resided for two years; and at the expiration of this time, he settled in general practice at Berkeley, under the roof of his elder brother, Stephen. The character and pursuits of Mr Hunter had a favourable influence on his pupil. The attachment between them was strong and tual; they were kindred spirits; and an uninterrupted episto correspondence was kept up by them till within a short perio Mr Hunter's death.

“ It was a truly interesting thing,” says Dr Baron, “ to hea Jenner in the evening of his days, descanting, with all the fer of youthful friendship and attachment, on the commanding and gaging peculiarities of Mr Hunter's mind. He generally called the ‘ dear man,’ and when he described the honesty and wa of his heart, and his never-ceasing energy in the pursuit of k ledge, it was impossible not to be animated by the recital, and to ceive that something more than esteem for high intellectual at ments, was required to form that bond of union which, to the hour of his life, joined the affectionate recollections of the pupil the memory of the master.”—P. 10.

While he was with Mr Hunter in 1771, Captain Cook ret ed from his first voyage of discovery, and Dr Jenner was em ed, on the recommendation of Hunter, to arrange and prepar valuable specimens of natural history which were brought ove Sir Joseph Banks. He was thus enabled to obtain much sk the dissection of tender and delicate organs, and in minute jections; and Dr Baron mentions a preparation of the progre the ovum in our common domestic fowl, which was bequeath him by Dr Jenner, and exhibits his manual dexterity in an mirable degree.

Dr Jenner was offered the appointment of Naturalist to Ca Cook's second expedition, which sailed in 1772, but decl it, as he did likewise an offer, said to be of emolument and tinction in the East Indies; as well as a connection with Hunter, in a school of natural history, which the latter prop to form in London, about the year 1775, and which required appropriation of 1000 guineas on the part of Dr Jenner, in first instance. The offer made to him of an appointmen the East Indies, was the result of the high opinion enterta of him by a gentleman of much political influence, in consequ of a little accident to which he was witness, and which we narrate. “ He was dining with a large party at Bath, whe question arose whether the temperature was highest in the ce of the flame of a candle, or at some small distance from its a Various opinions were delivered, but Jenner, with his usual genuity and readiness, soon settled the dispute. He placed candle before him, and inserting his finger into the middle of flame, he retained it in this situation for a short time. He t placed it a little above the flame, but was compelled immedia to withdraw it. ‘ There, gentlemen,’ he observed, ‘ the ques is settled.’” We should recommend the learned editor to try experiment himself; and we suspect he will find the callous practised finger of the sugar-baker necessary, to resist any modification of the flame mentioned, except with the rapid movement well known to be successfully employed in handling a red-hot cinder depositing itself on a *cara sposa's* favourite hearth-rug.

The mode in which Dr Jenner conducted his practice at Berkeley, was an agreeable and interesting one. His company was so much liked, that friends would often accompany him in his long rides, for the gratification of his conversation. "He never met with any one without endeavouring to gain or to impart instruction. In natural history, in particular, he wished from his earliest years to show how much information and amusement lie scattered around us, how bountifully the sublimest sources of gratification are supplied, and how desirable it is that all should be taught to taste them.

"With some of his particular friends he often, at this period of his life, spent days in their houses, especially in cases of sickness of a serious nature. In this way he made their home for a season his head-quarters, and from thence went to visit his patients in the surrounding district. In a situation like the Vale of Gloucester, this temporary sojourning at a few miles distance from his own abode, was not attended with much inconvenience in a professional point of view, and it was often a source of recreation and amusement to himself,—and I may add of unmingled satisfaction to his friends."—P. 14.

The second chapter of Dr Baron's work is principally made up of letters from Mr Hunter to Dr Jenner, between the years 1773 and 1783. These will be perused with interest, because they evince not only the high opinion entertained of the latter, but the nature of Hunter's pursuits, and the zeal with which, amid his increasing professional avocations, he still kept in view the cultivation of natural history. It was in May 1777 that Mr Hunter first mentioned, in a letter to Jenner, an attack of swimming in the head, which prevented him raising it from the pillow for ten days. He went to Bath in August, where he continued till November, and during his residence there, Dr Jenner was led to suspect the existence of *Angina pectoris*, by which Hunter's valuable life was terminated in the year 1793. He communicated his impressions the following year, in a letter to the late Dr Heberden, which, from some cause or other, was not forwarded; but the substance of the communication was published in a work by Dr Parry of Bath in the year 1799.

Dr Jenner belonged to two medical clubs, in one of which the *medico-convivial* and the members dined together in different places according to their convenience. In the latter, the *convivio-medical*, the meetings were principally held at the Ship at Alveston, about ten miles from Bristol. At meetings of the former, Dr Jenner's paper on *Angina pectoris* was read, which forms the ground-work of Dr Parry's book on that subject, and many communications made by him in physiology and pathology. Some of the latter fell into the hands of members who never returned them;

and Dr Jenner particularly lamented the loss of one of
which contained observations on a disease of the heart, whic
quently comes on during attacks of acute rheumatism, and
to enlargement and disorganization of that organ. This
have established his claims to priority in the description o
formidable and interesting complication of disorder.

At both these meetings, Dr Jenner was accustomed to
forward the reported prophylactic virtues of cow-pox against
pox, which existed in the country, and earnestly invited the
tion of the members to the subject. But though some of
were acquainted with the rumours, his efforts were unavailing
the topic became at last so distasteful, that they threatened
pel him if he continued to harass them with so unprofita
subject. Mr Fewster of Thornbury was one of the memb
the *convivio-medical* club, and was early associated with M
ton as an inoculator. The assertion, that he had any claim
regarded as the inventor of vaccination, is, however, entirely
out foundation; for in a letter of his, published in 1798, he s
in much discouragement of cow-pox; and Dr Baron recei
communication from him in 1823, which contained no sort o
timation of any such claim. An agreeable anecdote is giv
Dr Jenner being, at this time, so much in estimation as a sur
that, about the year 1778, he was sent for to the Hospital of
cester, to perform an important operation, (which he did suc
fully,) when both the surgeons were so circumstanced as to b
able to render their assistance.

About this time, he suffered severely from some disapp
ment in his affections, to which, at various times, he feeli
alluded in letters to confidential friends; but some years a
wards, namely, on the 6th of March 1788, he formed a h
union with Miss Catharine Kingscote, a lady to whom he
been long attached, and who belonged to an ancient famil
Gloucestershire, that was related to the great Sir Matthew H
From the year 1783, up to the period at which his Inquiry
published, Dr Jenner divided his time between the pursui
his profession, and attention to many objects of natural hist
in which he was in frequent correspondence with Mr Hun
He made an attentive and successful investigation, during
period, into the natural history of the cuckoo, the result of w
was communicated to the Royal Society, by Mr Hunter, on
13th of March 1788, and was published in the Philosoph
Transactions for that year. In this paper, Dr Jenner satis
torily proved, that the cuckoo deposits its eggs in the nest of
hedge-sparrow, or other small birds; that the hatching of
two sets of eggs goes on together, and is completed about
same time; and that at the period of hatching, the size of
sparrows, and of the intruder cuckoo, are nearly alike, but t

the equality does not long continue, and when the cuckoo has acquired sufficient size and strength, he contrives, by a singular configuration of his own body, to lodge his companions, one by one, upon his back, and, scrambling up the sides of the nest, completely ejects them. This procedure is sometimes carried on even under the eyes of the unnatural parent, who seems to be only anxious for the well-doing of her foster-child. When two cuckoos happen to be hatched together, there is a struggle for mastery, which ends in the defeat and ejection of the weaker. The final cause of this singular anomaly seemed to Dr Jenner to be, the early departure of the parent, which leaves the country in the first week of July, and the necessity of its young having maternal aid after this period. The young cuckoos go off, in succession, as soon as others are able to take care of themselves.

Dr Jenner was elected a Fellow of the Royal Society the year after the appearance of his paper, and, as he found the fatigues of general practice become irksome, he obtained the degree of Doctor of Physic from the University of St Andrew's, in 1792, and from that time, discontinued the practice of surgery. Dr Jenner paid considerable attention early in life to the nature of hydatids, and the early formation of tuberculous disorganizations. He was of opinion that the latter class of diseases were often erroneously referred to the existence of inflammatory action.

Mr Hunter was carried off suddenly, at St George's Hospital, about two months after the date of his last letter to Dr Jenner, of August 12, 1793; and Dr Jenner himself had a narrow escape from typhous fever towards the conclusion of the following year. Up to the 4th Chapter of his work, Dr Baron is principally occupied in delineating the character, and in describing the mode of life, the habits, and the avocations of Dr Jenner; but he now enters upon a consideration of the origin and progress of vaccination, and commences with its early history.

It appears that, during Dr Jenner's pupilage at Sodbury, a young countrywoman came to seek advice; and when the subject of small-pox was mentioned in her presence, she observed, "I cannot take that disease, for I have had the cow-pox." This circumstance made a great impression upon him, and was never subsequently lost sight of. He mentioned the subject to John Hunter in 1770, who made known in his lectures, and in conversation, the opinions of Jenner, and the traditions of Gloucestershire; but the medical men of the country, whose attention Dr Jenner was anxious to engage in the investigation, viewed the reports as having no real foundation, and as depending on some peculiarities of constitution, and not on any real prophylactic powers which cow-pox might possess.

It was not till the year 1780, that he was enabled "to unravel many of the perplexing diversities and contradictions with which the question was enveloped;" and in the month of May of that year, in riding with Mr Gardner, a particular friend, between Gloucester and Bristol, near Newport, he communicated a full statement of his ideas of the origin of cow-pox "from the heel of the horse; specified the different sorts of disease which attacked the milkers when they handled infected cows; dwelt upon that variety which afforded protection against small-pox; and with deep and anxious emotion mentioned his hope of being able to propagate that variety from one human being to another, till he had disseminated the practice all over the globe, to the total extinction of small-pox. The conversation was concluded by Jenner in words to the following effect:—'Gardner, I have entrusted a most important matter to you, which I firmly believe will prove of essential benefit to the human race. I know you, and should not wish what I have stated to be brought into conversation; for should any thing untoward turn up in my experiments, I should be made, particularly by my medical brethren, the subject of ridicule—for I am the mark they all shoot at.'"—P. 128.

Many years elapsed before he had an opportunity of completing his experiments on vaccination, in which he encountered many difficulties. These arose, in the first place, from his finding that there were two descriptions of cow-pox, a true and a spurious; and that in the former, if the virus were taken after a particular period, it had only the power of producing a local disease, and not that of imparting to the constitution a protecting property against variolous influence.

In 1788, Dr Jenner carried with him to London, a drawing of the disease, as seen on the hands of milkers, and communicated freely his opinions on the subject, as appears from various abstracts of letters; but it was not for eight years afterwards, that he was able to carry his ideas into effect, when he was at length, on the 14th of May 1796, gratified with an opportunity of communicating the disease (which was done by two superficial incisions) from the hand of Sarah Nelmes, to the arm of James Phipps, a healthy boy of about eight years of age. He went through the disease in a regular and satisfactory manner; and on the 1st of the following July, was inoculated with matter immediately taken from a small-pox pustule. Dr Jenner had the indescribable gratification of finding, that no disease followed, and communicated the happy tidings to his friend Gardner, in a letter dated July 19. About the same time, he offers, in some memorandums, a picture of feelings, "full of interest and full of piety," with which he was actuated, in consequence of the brilliant prospects of reputation and usefulness which were thus opened to him.

Owing to the disappearance of cow-pox from the dairies, Dr Jenner was unable to prosecute his researches till the spring of 1798, when he repeated his inoculations with ease, and prepared his work for the press. As soon as this was ready, it was submitted to the scrutiny of a small number of particular friends, and by their advice it appeared as a distinct publication about the end of June, instead of being presented as a communication to the Royal Society, as was originally contemplated. It appears, indeed, from some observations contained in the second volume, that Dr Jenner was anxious for his first communication, relative to vaccination, to appear in the Philosophical Transactions; but that he was deterred from formally presenting it to the Royal Society, (for some notes of it had been submitted to Sir Joseph Banks and the Council,) by being given to understand that Sir Joseph advised him to be "cautious and prudent" in the business; and stated, "that he ought not to risk his reputation, by presenting to the learned body anything which appeared so much at variance with established knowledge, and withal so incredible." And thus did the Royal Society lose the credit of being the original medium of announcing the discovery of vaccination to the public. He went to London previous to the publication of his first work, and remained there from the end of April to the 24th of July; but during the whole of that time, he was unable to prevail on a single individual to submit to vaccination.

Mr Cline, in the end of July, inserted some vaccine virus, by two punctures into the hip of a boy, having some disease on the joint, with the view of its acting as a counter-irritant. The ulcers were not large enough to contain a pea, and were not, therefore, converted into issues, as was intended; but on inoculating the boy with small-pox matter, no effect followed, but a very slight inflammation, which subsided in three or four days. In consequence of the success of this experiment, Mr Cline very strongly advised Dr Jenner "to quit the country, and to take a house in Grosvenor Square, and promised him L. 10,000 per annum, as the result of his practice;" and his opinion was supported by that of Sir Walter Farquhar; but he was too much attached to the peculiar enjoyments of the country, to be moved by considerations of fame and fortune; and in a letter dated September 29th, he pourtrays his feelings on the subject, in a highly interesting manner.

A letter from the late Francis Knight, Esq., Inspector-General of Army Hospitals, mentions about this time his knowledge of the fact, that, in the dairy parts of Wiltshire and Gloucestershire, where he had long resided, the power of an attack of cow-pox in rendering the constitution unsusceptible of small-pox, was a matter of belief among the dairymen; and it may also be stated, that in a note-book of Dr Jenner, of 1799, is a memorandum to this effect, that the celebrated Duchess of Cleveland, on being taunted by her companions, that she might soon have to deplore the loss of her beauty by the small-pox, which then raged in London, replied, "that she had no fear about the matter, for she had had a disorder, which would prevent her from ever catching the small-pox."

Four of the chapters of Dr Baron's work, are now devoted to a critical examination of the opinions of Dr Jenner, respecting the identity of the *variolæ*, and *variolæ vaccinæ;* and, as much industry and talent have been displayed in collecting the materials by which Dr Jenner's views are supported, we feel it necessary to give a general statement of the argument.

Dr Jenner always considered small-pox and cow-pox as modifications of the same disease; and imagined that, in employing vaccine lymph, we only use means to impregnate the constitution with the disease in its mildest instead of its most virulent form. From this view of the subject he was induced, in adopting the term *variolæ vaccinæ*, to designate the next form of disease by a modification only, or the appellation given to the old one. In pursuing the argument, Dr Baron lays down the two following propositions, which he considers as proved by the evidence brought forward; namely, "First, that an eruptive disease common both to man and to the inferior animals has been known in different ages, and in different countries; and that the descriptions given of this eruptive disease by various writers accord so completely with those acknowledged to be characteristic of small-pox, as to render it highly probable that this disease actually existed at a much earlier period than that usually assigned to its origin.

"Secondly, that as there are numberless writers who have described the small-pox in man, so there are others of established name and reputation, who have treated of a similar eruptive and pestilential disease as existing in various countries and in different times among the inferior animals, but especially among cattle; that to this disease they have unhesitatingly applied the name of variola; and actually recommended such treatment as experience had proved to be useful when that disease attacks man."—P. 163.

From these propositions he considers the position fairly deducible, "that the variolæ of men, and of the inferior animals, are essentially and originally the same; and that, from their first appearance to the present time, they have existed under various modifications."

Dr Baron goes into sacred history for the first point of evidence in support of his first proposition, and considers the character of the disease brought on the Egyptians by the sprinkling of ashes by Moses, which consisted of "boils, breaking forth with blains upon man and upon beast," to be identical with the small-pox of modern times. He quotes Dr Willan as supporting this view of the subject, and gives a passage from Philo Judæus, which communicates a lively, and, he conceives, an accurate description of that disease as a sort of corollary on the original account. He then offers a succinct general sketch of various pestilences common to man and beast, which have occurred in various parts of the world, from the earliest periods of Roman and Grecian history, down to the reign of the Emperor Commodus; and afterwards directs his attention to those historical documents which seem to him to prove that small-pox has at various times affected the inferior animals as well as man. The account given Thucydides, of the plague of Athens, he mentions as confirmatory of what has been already adverted to in respect to Philo Judæus and Egypt; and he adduces, likewise, the history of a similar complaint characterized by pustules, as that was which occurred in the army of Artaxerxes. He also considers the description given by Hippocrates in the 6th book of his Epidemics, as applicable to a malady which can hardly be regarded but as identical with small-pox. In China and Hindostan, small-pox has been known from the remotest antiquity; and the elder Pliny adds his testimony to the accumulated evidence of ages, in favour of the opinion, that many eruptive diseases were traceable to Egypt; and that this country was known to have had a very early connection with the ancient inhabitants of China and India. To connect the eruptive diseases now mentioned, with those which are known to have extended their ravages over the world, subsequent to the Christian era, Dr Baron resumes his sketch of epidemic diseases, of which he gives a very graphic account from Eusebius, Procopius, Evagrius, and others, up to the sixth century, when small-pox is usually considered as having first made its appearance at the siege of Mecca. From the time now mentioned, till a comparatively late period, the histories of epidemics have been given with much vagueness and uncertainty; but they still afford much presumption, as Dr Willan has shown in his account of the antiquity of small-pox, that in many of them, small-pox, and not the real plague, was designated. Dr Baron now follows up his inquiry, by offering the testimony of authors who have written on the diseases of cattle, to show that an epidemic small-pox has been often known to rage among them.

Lancisi, in his Treatise *de Bovilla Peste*, describes a disease which was epidemical in the papal territory in 1713–14, and which was similar to one described by Fracastorius, as occurring in Italy two centuries before. It was attended by pustules resembling those of small-pox; and the disease described by Ramazzini, as seen by him in the years 1690 and 1711, possessed the same character. The propagation of the latter by contagion was proved by the disease having been communicated accidentally through the means of a single strayed ox being thoughtlessly brought among an uncontaminated herd. Virgil, Ovid, and Lucretius are quoted as describing epidemic distempers among cattle; and Goëlicke, in his Dissertation *de Lue Contagiosa Bovillum genus nunc Depopulante*, (1730,) gives a similar train of symptoms to those mentioned by Lancisi and Ramazzini, and traces this contagious malady from the confines of Tartary into Europe, through Muscovy and Poland. Later authors, and particularly Dr Layard, have given more precise and satisfactory statements of diseases among cattle than had been done previously; and the latter describes an eruptive disease, having "all the characteristic symptoms, crisis, and event of the *small-pox;* and whether received by contagion, or by inoculation, has the same appearances, stages, and determination, except more favourable by inoculation, and with this distinctive and decisive property, that a beast having once had the sickness, naturally or artificially, never has it a second time." From this quotation, it appears that Dr Layard found the disease communicable by inoculation from one animal to another; and M. Viborg of Copenhagen made the same discovery in respect to small-pox from the human subject to dogs, apes, and swine.

The first recorded case of small-pox, under an unequivocal designation, is stated to have been about the year 907, in the person of Elfrida, daughter of Alfred, and wife of Baldwin, Earl of Flanders; and about this period the disease appears to have obtained the appellation of Variola. By the more early writers, it was regarded as a disease which might affect the same individual more than once; but, in consequence of Mead promulgating an opinion of a different kind, it was erroneously concluded, that wherever it was said to occur a second time, the second attack was to be regarded as merely one of chicken-pox, swine-pox, or some other eruptive disease. Mead's opinion was, however, soon disproved by the death of Louis the Fifteenth from small-pox, at the age of 64, after having undergone that disease at the age of 14. This event made a great sensation; and the author can refer to more than 130 different writers who have given similar examples.

The introduction of variolous inoculation into England, through the discrimination and energy of Lady Mary Wortley Montague, made a new epoch in the history of small-pox; but the diffusion of the infection through the means of this process formed a most forcible objection to the practice of inoculation.

The general sketch of Dr Baron's researches which we have thus given, will afford our readers a slight means of forming some opinion as to the strength of the evidence in favour, both of the common origin of small-pox and cow-pox, and of several domestic animals being equally subject to the one disease as the other. "The existence of *variolæ vaccinæ,*" says the author, "in the dairies of England, would seem not to have been of very long duration. I think there is good ground for believing that the disease, as originally noticed by Dr Jenner in Gloucestershire, was the endemic or local remains of the more general or epizöotic disease which prevailed in many parts of this island, at the period when Dr Layard wrote." This opinion he states to be confirmed by the confirmation, that cow-pox has been found in various parts of England, and also in Italy, Holstein, France, and Spain; and in Persia, the East Indies, and Peru. At first, Dr Jenner considered the matter of grease as requiring some modification, by passing through the constitution of the cow, before it was capable of producing genuine cow-pox in the human subject; but this process he afterwards regarded as unnecessary, and considered it able to produce the true cow-pox at once. At the latter period of his life, Dr Jenner found reason to alter, in some respects, his sentiments in respect to the grease, which he was at first disposed to consider as absolutely necessary for the production of cow-pox in cows. He regretted, therefore, the employment of the term *grease*, and wished to have characterized the complaint by the designation of *variolæ equinæ*. This he considered to be often co-existent with grease (which has been long a well-known disease among horses), but as having a totally different character, being an eruptive disease, and having vesicles resembling those which appear in the cow.

At the commencement of his researches, Dr Jenner's opinions as to the power of a disease in the heels of horses producing the cow-pox in the cow, was a good deal hypothetical; but in the year 1817, he had an opportunity of producing a true vaccine pustule from a variolæ equinæ pustule in the horse; and this was capable of propagating the disease in the same manner as is done by lymph from the cow. Dr Baron states, as an important confirmation of these experiments, that in the Almanac de Carlsbad, for 1833, is a communication from Dr M. C. G. Kahlert, Assistant Professor of Veterinary Medicine, at Prague, in which he mentions the occurrence of the variolæ equinæ, in Bohemia, and the power of producing genuine cow-pox through its means.

In some few instances, the cow-pox seems to have been as severe a disease among the dairy people, as the inoculated small-pox; and hence Dr Jenner derived a further point of analogy with small-pox itself: for after a series of inoculations, the severity of symptoms, and the number of pustules, have sometimes so much diminished, as that one only was seen at the point of insertion of the variolous matter, which was sufficient for producing perfect safety from a future attack of small-pox. It is likewise stated, that the late Dr Adams, when physician to the Small-pox Hospital, thought he had succeeded in producing, by means of careful selection from mild cases, a benign form of variolæ, which he termed *pearly-pox*. This form of disease was attended with scarcely any eruption or constitutional affection, and Dr Adams expected that it would be capable of perpetuation; but the introduction of cow-pox was probably the means of putting aside any further experiments or observations on the subject.

In taking a retrospect of the evidence which has been so carefully adduced, to prove the identity of nature between the small-pox and cow-pox, we must be allowed to observe, that the argument deducible from Sacred Writ appears to us as somewhat irrelevant and inapplicable. The production of a disease by the sprinkling of ashes of the furnace, was an operation of Divine agency, and was hardly, we would submit, likely to be characterized, either by a series of symptoms at that time known, or by phenomena which thenceforward obtained any fixity, or permanence of character. Maladies which arise from natural causes are submitted to certain laws which nature has doomed them to follow, and with which, by careful observation, we may become acquainted; but miracles, we need hardly say, are placed beyond the sphere of human research; and we should deem it as fitting a subject of inquiry, to consider to what genus of Willan and Alibert were the "sore boils" referable, with which Job was visited "from the sole of his foot to his crown," as the attempt to reconcile any of the plagues of Egypt to the divisions of modern nosology. The infliction of blains and boils was a noisome and offensive visitation; but we are not told that it was a fatal one, which it must have been, if it bore any relation to the usual character of small-pox. Nor could we have any ground for imagining, except on the supposition of a supernatural operation, that a disease would pass over the period of its usual progression, and appear at once in full maturation and loathsomeness. Dr Baron, indeed, considers his view of the argument as strengthened by the circumstance of the disease not having been "removed from the sufferers, as some of the other inflictions were, by Divine interposition, but were left to its natural course, and thus may have been propagated, through successive generations of mankind, as we know has been the case with small-pox." But even in this view of the subject, we submit, that Dr Baron's reasoning cannot be regarded as conclusive. What a miracle could bring on, a miracle could carry off; and there is every reason to believe that such was the case; for if it were otherwise imagined, from any supposed omission in the narrative, then it would follow, in reference to the first of the plagues, that the rivers, ponds, and pools of water would remain blood till a spontaneous purification might take place; while in the third, the lice, into which the dust of the earth was converted on man and on beast, would continue to infest the Egyptians, and not be superseded by the flies which succeeded them. It was not actually said that they were removed before the appearance of this new scourge; but yet this is almost a necessary supposition; for when it was entreated that the flies should be withdrawn, it could not be imagined that the lice would, if not previously removed, have been left. We feel that somewhat of an apology is due for touching at all on points which we consider to be so little adapted for the exercise of ordinary judgment and discrimination, as those connected with the historical narrations of Holy Writ; and being anxious to take leave of this part of the subject, we would only further remark, that the land of Goshen, which the children of Israel inhabited, is not always stated to have been exempt from the visitations of the Egyptians, though this must always be understood to be the case.

The appellation of *Variolæ Vaccinæ*, which Dr Jenner gave to the cow-pox, would have been a very unexceptionable one, even without the supposition of the original identity for which he contends; but we do not see why the plural denomination should have been employed by him instead of the singular, for in the case of small pox, variola, though singular, designates, a disease indicated by pustules, as the best marked and most characteristic phenomenon; while in cow-pox, there is but a single pustule, and that only existing at the place of vaccination. The analogies which Dr Baron adduces in favour of the identity of small-pox and cow-pox are highly interesting, and eminently creditable to his industry and talents; but it hardly appears to us, that the admission of the full extent of his conclusions would, as Dr Baron seems to think, give any important additional "stability" to vaccine inoculation, or secure for it a confidence which it would not have otherwise had. Indeed he states, in one part of his work, his feeling, that "Dr Jenner's opinion respecting the origin of cow-pox is comparatively of little moment, when contrasted with the important consequences arising from the successful practice of vaccine inoculation;" and we agree with him in his sentiment on this point; for if no such idea of the probable origin of cow-pox had been entertained, the prophylactic power of its operation on the human constitution would have stood as a simple fact, to be ascertained by full careful investigation. It was to its bearing out the inquiries thus made, that its early and rapid progress was to attributed; and that, even in the face of a very general indisposition to believe in its origin from the heels of the horse. But, granting in its fullest extent a community of nature between cow-pox and small-pox, there seemed to be no reason for supposing, that the mitigated form of disease which constituted the former, was produced or kept up in the horse more readily than in the cow; and this is the opinion which, as we have already stated, Dr Jenner, with his usual candour and love of truth, adopted in the latter years of his life.

In considering the evidence adduced by Dr Baron, from the history of eruptive epidemics, it does not appear to us that there has been usually any thing of sympathy or interchangeable character, existing between the diseases affecting man and the lower animals. The pestilences described by Livy, and other ancient authors, as occurring previous to the commencement of the Christian era, are, it is true, stated to have affected both man and beast; but the eruptive complaints which are described as occurring subsequently to that period, and which Dr Baron considers as referable to small-pox, are, it appears to us from his work, with one exception, and that given on the authority of St Ambrose and Paulinus Nolanus, confined to man.

The epidemic variolæ of more modern times have not been communicated from man to the brute creation; nor have the eruptive contagious diseases of cattle been transferred to man. So far, therefore, their respective diseases seem to have been separate and distinct; and it is very difficult, therefore, to go to the full extent of the author's conclusions, in respect to the complete original identity of some of them. There are no circumstances of analogy with which we are acquainted, which appear to us to justify a belief that a contagious disease occurring but once is capable of losing its contagious character; of becoming mild and innocuous; and having the power of preventing in another class of animals, whose diseases are not interchangeable, the occurrence of a complaint resembling that, in some of its phenomena, from which the milder one was supposed to have arisen. The grease, or rather we ought to say the *variolæ equinæ*, in horses, and the cow-pox in cows, occur, we presume, several times in the same animal; though when transferred to man, they singularly enable his constitution to resist a second production of the same affection.

In supposing that the *variolæ vaccinæ* in the dairies of England, is the endemic or local remains of the epizootic disease described by Dr Layard, a very recent origin is attributed to vaccine influence, hardly, it might be supposed, sufficient to produce that general feeling in favour of its prophylactic powers against small-pox, with which Dr Jenner accidentally became acquainted (as has been already stated) previously to the year 1770. This supposition would likewise be adverse to the idea said to be entertained by the Duchess of Cleveland on this subject, and it would be very singular that the existence of cow-pox in many parts of England, as a remains, it is also to be imagined, of the epizöotic disease above-mentioned, should have failed to generate a conviction of the same preventive influence, had such an influence been at that time actually exercised. The same observation may likewise be made respecting similar complaints which have appeared in various parts of Europe. Those of the East Indies seem to be of a more severe character; and in proof of this Dr Baron gives from the article by Mr M'Pherson in the sixth volume of the Calcutta Medical and Physical Transactions, already reviewed in the forty-fourth volume of this Journal, the account of the cow-pox to which the cows are very subject in that region, and from which he was able to produce the real prophylactic cow-pox. The severity of the disease prevented the cows from giving milk during its existence; and the milkers had not, therefore, the chances which occurred in other countries, of contracting the disease.

If the single pustule stated by Dr Jenner to have sometimes occurred at the place of insertion of variolous matter, were disarmed of its power of producing more than a single one in a future transfer, and of propagating the disease by contagion, there would be much additional support given to the supposed production of those changes, which are necessary for the complete admission of the author's theory. It may likewise be observed, that, long as small-pox has existed to be the scourge of mankind, no such spontaneous modifications of its phenomena have ever exhibited themselves, as have been supposed to take place in epizöotic diseases, notwithstanding a milder form of the disease, by inoculation, has long existed in this country, and has been known, time immemorial, among some of the Eastern nations.

We make these observations with a strong feeling of the force of Dr Jenner's and the author's reasoning; and a perfect conviction of the important bearings of the facts by which it is supported; facts, it must be observed, which have the more interest and value, by their having been derived from such numerous and unconnected sources.

Soon after the appearance of his inquiry, Dr Jenner was much annoyed by the receipt of a letter from the celebrated Dr Ingenhousz, who was then on a visit to the Marquis of Lansdowne, at Bowood, in which he threw many doubts respecting Dr Jenner's conclusions relative to the effects of vaccination, from inquiries made among the dairies in Wiltshire, and from an imperfect acquaintance with the nature of the vaccine vesicle. In rebutting these objections, which were urged in a manner that was not perfectly agreeable, Dr Jenner found it necessary to distinguish between the true and false cow-pox pustule in the cow; and when he published his further observations on the variolæ vaccinæ, on the 5th of April 1799, he took occasion to advert to Dr Ingenhousz's communications. At this time, he complained much of pressures of the mind growing too heavy for him; and stated that, in addition to all his other cares, he was touched hard with the epidemic, impecuniosity.

The successful experiments made by Mr Cline in the summer of 1798, had a very favourable influence on vaccination; but it was not till November of that year, that Dr Jenner was able to procure a supply of vaccine lymph, with which to resume his operations. About this time, vaccination was going on to a considerable extent in London, by the exertions of Dr Pearson of St George's Hospital, and of Dr Woodville of the small-pox; but in so many instances, a pustular disease was produced, as to make Dr Jenner suspect that "the London cow-pox was somehow or other compounded with small-pox." This proved to be the case; though when Dr Jenner employed in the country some of the matter transmitted to him from Dr Woodville, the first vaccinations were attended with a few small spots only on the arms and face; yet the subsequent ones had only the proper local appearances; while all were exposed to small-pox contagion without effect. From this transplanted matter, Dr Marshall began his vaccinations, which were continued with genuine cow-pox virus, obtained from a farm at Kentish Town, by Dr Jenner, during a residence in London from March 1799 till the following June.

Amid the prejudices which were excited in many quarters against cow-pox by the report of its being an eruptive disease, it continued, however, to gain ground. Many of the higher orders of society had their children vaccinated, and particularly the Duke of Clarence at Bushey, two of whose children were subjected to the complaint, through the advice and agency of Mr Knight.

The transmission of contaminated matter to the country by Dr Pearson, and the consequent production of a pustular disease at Petworth, occasioned an interesting correspondence with Lord Egremont on the subject; but Dr Jenner forwarded some of the proper virus, with which forty were vaccinated, and all had the disease in the regular form. It appears, however, that the production of pustules did not invariably result from the employment of the London virus; for that which we have just mentioned, as having been transmitted by Dr Woodville, had no such effect; and the same may be remarked likewise in respect to some that was forwarded to Dr Jenner by Dr Pearson, in the early part of March, not long before the former set out for London.

It was a gratifying circumstance to Dr Jenner, now to be apprized of the deep interest with which his discovery was received at Geneva, Hanover, and Vienna; and almost simultaneously in the United States of America, (where, with characteristic simplicity, it was communicated by Dr Waterhouse, in an article headed, "something curious in the medical line,") Newfoundland, France, Spain, the Mediterranean, through Gibraltar and Malta, Constantinople, Bagdad, and the East Indies. It was subsequently introduced into Denmark, Sweden, and Russia. Sir Gilbert Blane, as one of the Commissioners of the Sick and Hurt Board, recommended, in 1800, the introduction of vaccination into the Navy; and Dr Trotter, physician to the Fleet, not only seconded the measure with every degree of zeal and energy, but in the following year, joined with the other medical officers of the navy, in presenting to Dr Jenner a gold medal on his discovery, on which Apollo is represented "introducing a young seaman to Britannia; who in return extends a civic crown, on which is written Jenner:"

above, "Alba nautis stella refulsit," below, "1801." On the reverse, an anchor; over it, "Georgio Tertio rege;" and under it "Spencer duce." In the same year he received a most gratifying testimonial of regard from his own country, in a small service of plate presented to him from the Earl and Countess of Berkeley, and his other Gloucester friends.

Soon after the publication of Dr Jenner's first work, in 1798, an unmanly and ungenerous attempt was made, in a quarter where he did not at all expect it, to call off the public attention from the merit to which he had so undoubted a claim, and to force into notice an active and unlooked for competitor for vaccine reputation. During his residence in London, Dr Jenner was open and communicative on the subject of his discovery, with every member of the medical profession; and finding Dr Pearson much interested in the progress of vaccination, gave him the most unreserved information on every matter of consequence which was connected with it. Dr Pearson soon set about establishing an extensive correspondence with medical men in different parts of the kingdom on the subject of the new discovery; and so early as the following November, published a pamphlet, which contained, with some reasoning of his own, the result of the information which he had thus obtained. He co-operated zealously with Dr Woodville, in carrying on vaccination at the Small-pox Hospital; and circulated extensively through the country, an account of the result of their experiments; inclosing portions of thread imbued with the vaccine virus, in order to diffuse the means of carrying on the practice. The most high-spirited and dignified course of professional procedure, would perhaps have been to co-operate with Dr Jenner in prosecuting his researches, and not to treat vaccination as a sort of loose fish, to be followed and secured without reference to the original author. But so little feeling of jealousy had the illustrious discoverer, where the cause of vaccination was concerned, that every independent exertion in its favour was fostered and encouraged in whatever quarter it might be made. Dr Pearson's conduct at this time, however, roused the attention of some of Dr Jenner's friends, for he not only gave a public lecture on the subject, but prepared letters for general circulation, in which he offered to supply medical gentlemen with vaccine matter on application to him; thus setting himself up as the chief person to be known in the business. Dr Jenner's visit to London in March 1799, (which he made in consequence of the necessity of its being strongly urged upon him) seems to have checked some of the proceedings which were thus intended; but soon after his return home, in June, Dr Pearson set about forming a vaccine establishment, in which he himself was to have the chief place, and obtained the sanction and cooperation of the late Duke of York, and Lord Egremont, both of whom agreed to take office in it. Other high patronage followed, and then came the formation of an extensive medical staff, of physicians of the college, consulting physicians, surgeons, and visitin apothecaries, all owing their appointments to the patronage of D Pearson, and all of them of course gratified in being the object of his selection and favour. As soon as the plan was matured and when it could not be longer concealed, Dr Jenner was appri ed of its formation by a letter from its prime architect, which w view as a very singular, but not less palpable specimen of disin genuous composition. In respect to the new establishment, D Woodville seems to have been felt as a sort of dead weight in th arrangements, for Dr Pearson says of him, that " exactly what D Woodville will or can be, on account of his connection with th Small-pox Hospital, I cannot tell;" but as to Dr Jenner, he ex perienced less difficulty, and most liberally offers to recommen him for the appointment of " an extra corresponding physician," without any further expense being attached to the situation "ex cept a guinea a year as a subscription," and from which trifling su for so great a dignity, he thinks he ought to be exempted, "as," says he, " you cannot send any patients;" but then, recollecting that it would be very hard on the establishment to lose a guine for such a flimsy reason, he adds, " but you may depute som proxy in town."

A very just and dignified reproof was sent in reply, of whic our readers will be gratified in seeing a copy. It was as follows

"SIR, *Berkeley, Dec.* 17, 1799.

" I received your letter of the 10th instant, and confess I felt sur prised at the information it conveys.

" It appears to me somewhat extraordinary that an institution form ed upon so large a scale, and that has for its object the inoculatio of the cow-pox, should have been set on foot and almost completel organized without my receiving the most distant intimation of i The institution itself cannot, of course, but be highly flattering t me, as I am thereby convinced that the importance of the fact I imparted is acknowledged by men of the first abilities. But at th same time allow me to observe that if the vaccine inoculation, fro unguarded conduct, should sink into disrepute (and you must admit Sir, that in more than one instance has its reputation suffered) alone must bear the odium. To you, or any other of the gentleme whose names you mention as filling up the medical departments, i cannot possibly attach.

" At the present crisis I feel so sensibly the importance of the business that I shall certainly take an early opportunity of being in London. For the present I must beg leave to decline the *honour* intended me.—I remain, Sir, Your obedient Servant,

E. JENNER."

On Dr Jenner's going to London soon afterwards, and making known the improper mode in which he had been treated, the proposed institution fell to the ground; the Duke of York, Lord Egremont, and its other most able supporters, retiring in disgust.

Dr Jenner's constant devotion to the cause of vaccination, the expenses which a very extensive correspondence continually entailed upon him, and the abstraction thus necessarily made from his professional avocations, induced him and his friends early to think of the propriety of applying to Parliament for a pecuniary grant. A petition to this effect was accordingly presented by him to the House of Commons, on the 17th of March 1802, during the premiership of Lord Sidmouth, then Mr Addington, which led to a vote of L. 10,000, instead of a much larger sum, to which a large minority thought he was justly entitled. Much dissatisfaction was entertained very generally by the penuriousness of the grant; but Parliament redeemed its character some years afterwards, by a further vote of L. 20,000. The way was prepared for this by a motion made on the 2d of July 1806, by the present Marquis of Lansdowne (then Lord Henry Petty) for a reference to the College of Physicians, as to the importance and success of vaccination. In consequence of their report being exceedingly favourable, a proposition for a further grant of L. 10,000 was made on the 29th of July 1807, by Mr Percival, who succeeded Lord Henry Petty as Chancellor of the Exchequer. When this came under consideration, Mr Morris, the Member for Newport in Cornwall, and son-in-law to the Ex-chancellor, Lord Erskine, moved that the sum of L. 20,000, instead of L. 10,000, should be inserted in the resolution. The proposition was seconded by Mr William Smith, the member for Norwich, and was carried by a majority of sixty against forty-seven.

This gratifying result was the more important to Dr Jenner, on account of the circumstances to which the first pecuniary grant gave rise. Mr Addington was by no means disinclined to reward the valuable discovery of Dr Jenner, more liberally than by a grant of L. 10,000; but then he considered that there would be a full pecuniary compensation, " by means of the sanction" of the House, in the increase of his professional emoluments; and spoke confidently of the " comfort of his family" being " amply provided for, in his extended practice." In consequence of the strong opinion expressed in such a quarter, (in which many of his Parliamentary advocates joined,) Dr Jenner was induced, soon afterwards, to fix himself in Hertford-Street, May Fair. The result of the plan, however, by no means corresponded with the anticipations which he had been led to form; and Dr Jenner thus feelingly expresses his disappointment. " Elated and allured," he observes, " by the speech of the Chancellor of the Exchequer, took a house in London for ten years, at a high rent, and furnished it; but my first year's practice convinced me of my own temerity and imprudence, and the falsity of the minister's prediction. My fees fell off both in number and value; for, extraordinary to tell, some of those families in which I had been before employed, now sent to their own domestic surgeons or apothecaries to inoculate their children, alleging that they could not think of troubling Dr Jenner about a thing executed so easily as vaccine inoculation. Others, who gave me such fees as I thought myself entitled to at the first inoculation, reduced them at the second, and sank them still lower at the third." "The truth is," says Dr Baron, "that Jenner, in publishing his discovery as he did, effectually prevented the fulfilment of Mr Addington's predictions; and it was scarcely befitting the representatives of a great nation to speculate on a contingency of this nature, in calculating the reward due to such a benefactor. He himself remarks to one of his correspondents, 'I have now completely made up my mind respecting London. I have done with it, and have again commenced village-doctor. I found my purse not equal to the sinking of a thousand pounds annually (which has actually been the case for several successive years,) nor the gratitude of the public deserving such a sacrifice. How hard, after what I have done, the toils I have gone through, and the anxieties I have endured in obtaining for the world a greater gift than man ever bestowed on them before (excuse this burst of egotism,) to be thrown by with a bare remuneration of my expenses!'"—Pp. 3, 4.

It would appear that the remuneration voted was not actually paid for more than a period of two years, and then was burdened with fees, &c. to the amount of L. 1000. His emoluments from vaccine inoculation were not, Dr Baron informs us, on an average more than L. 350 per annum; and with the various expenses which he was under the necessity of incurring, without the possibility of a return, there was, therefore, a gross deficit of capital, in the four years immediately subsequent to his removal to London, of nearly L. 6000. It was highly honourable to the public spirit of the friends of vaccination in the East Indies, that very nearly L. 7500 were raised in the three presidencies, soon after the second grant, as an acknowledgement to Dr Jenner for his important services.

Early in 1803, Dr Jenner was gratified with the formation of the Royal Jennerian Society, which immediately obtained the gracious patronage of their Majesties, and the support of the other branches of the Royal Family, and of the most distinguished of the nobility and gentry. For some time, the business of this Society went on prosperously; but the irregularity exercised in respect to the vaccinations by Dr John Walker, the resident inoculator and medical secretary, gave much uneasiness to Dr Jenner, and ended in Dr Walker's resignation. The society lingered some time; but on the establishment of the National Society, it was no longer necessary, and may at that time be considered as having ceased to exist; though there was for some years afterwards, and still perhaps may be, a society under the same name, which never received any countenance from Dr Jenner.

Soon after the adjustment of the second parliamentary grant, government determined on founding an establishment for the propagation of vaccination throughout the British dominions. This was carried into effect in the year 1808, when a board was formed of the President, and four Censors of the College of Physicians, and the President and two Vice-Presidents of the College of Surgeons, with L. 3000 per annum to defray the expenses. These consisted of L. 100 per annum to each of the eight members, and of an equal sum to vaccinators, who were named for different parts of the town, together with adequate payments for directors and other officers, as well as for house-rent and incidental expenses. It would have been natural to suppose that Dr Jenner would himself have been nominated as a principal member of this board; and the office of its President would have been an appropriate one for him. With the semblance, indeed, of much attention to his feelings, he was named as *Director*. But when he found, that in this capacity, he was considered but in a subordinate view, while all the real management and control remained with the board, in whose deliberations he had no voice, he thought it advisable to relinquish that situation, though he was always willing to co-operate with, and assist the board, by every means in his power; and in numerous instances did so. Till the reform took place in Parliament, the board remained on the footing, and with the income of its original establishment; but since that period, the office of member of the board has, we believe, been made entirely honorary.

From some observations made in other parts of the work, it would appear, that objections were raised against the introduction of Dr Jenner into the Vaccine Board, from his not being in any way connected with the London College of Physicians. So little, however, often appears to be known about the real causes of public arrangements, that a right honourable gentleman (but whether a member of the government is not stated) was said by Dr Jenner, in a letter to Mr Moore, to account for this exclusion, from the circumstance of the Board, instead of being constituted to carry into operation a process already accredited and approved, (which it unquestionably was,) being nothing more than an institution formed for the purpose " of a full and satisfactory investigation of the benefits or dangers of the vaccine practice; and that this was the reason why Dr Jenner could not be admitted as one of the conductors of it, as the public could not have had the same confidence in their proceedings, as if the Board were left to their own judgment in doubtful cases."

Dr Jenner's own apprehension on the subject, seems, however, to have been expressed to Dr Baron, in a conversation held in 1817, when he said; "The present constitution of the National Vaccine Institution is bad. The Marquis of Lansdowne and myself had arranged an excellent plan; but the change of the ministry knocked it on the head; and George Rose and Sir Lucas Pepys concocted the present imperfect scheme." It would have been no more than an act of justice to Dr Jenner, as well as to the public, that he should have had a prominent position in an establishment, which owed its existence to his discovery. This exclusion was a perfect anomaly in public proceedings.

In the year 1811, an unfortunate circumstance occurred in respect to vaccination, which created a very great sensation at the time, and occasioned extreme uneasiness and annoyance to Dr Jenner. The Honourable Robert Grosvenor, a son of the Earl of Grosvenor, who had been vaccinated ten years previously, by Dr Jenner, was attacked with small-pox, and had it severely. It appears, however, that the latter stages of the complaint went on rapidly; that there was no secondary fever; and that, on these accounts, his recovery was to be fairly attributed to the influence exercised on his constitution by previous vaccination. None of the other children who had been vaccinated, although they were exposed to the contagion of small-pox, under which their brother was suffering, took the disease. A circumstance of this kind could hardly be considered as more than a very slight counterpoise against the important benefits which vaccination had diffused; and Dr Jenner, as well as the more dispassionate and considerate part of the public, viewed it in this light; so that vaccination continued to flourish, and the Vaccine Board, under the directorship of Mr James Moore, went on, contributing largely to the public benefit.

It was perhaps unfortunate for vaccination, that it was at first regarded as an almost certain prevention against small-pox, though it could hardly be expected to be more so than small-pox itself; and Dr Baron admits, that the original statements touching the absolute protection afforded by vaccination were too unconditional. As far a[illegible] Jenner was concerned, he had, up to this time, vaccinated for a long series of years, many thousands "without meeting with any interruption" to his success before; and his unbounded reliance on the efficiency of vaccination could not therefore be at all wondered at. It would, indeed, have been rather fortunate if he had, at the commencement of his career of usefulness, met with some of those cases of mild small-pox after vaccination, which occasionally were seen by the best and most candid observers, very early in the history of vaccination; for they would have been the means of modifying his opinions in respect to the cause of failure, which, up to the occurrence of Master Grosvenor's case, he generally and quite fairly represented as usually consisting in an irregularity in the vaccination itself, or the existence of some constitutional circumstance, which prevented its full operation.

A large portion of Dr Baron's volumes necessarily refers to the introduction and progress of vaccination in different countries of the world, and to the honours which flowed upon him from the grateful feelings of states, societies, and individuals. A list of these last tributes of respect and regard is given at the end of the second volume, and will be perused with much interest. In 1808, Dr Jenner was elected a Corresponding Member of the National Institute of France; but, in 1811, he had the further honour of being placed in the class of Foreign Associates, on the death of Dr Maskelyne. An intimation of this agreeable event was communicated to him, in a letter from Sir Joseph Banks, who at the same time complimented the institute, on preserving its original designation of *National*, without humbling itself to the " arbitrary dispositions of their sovereign, by accepting the preferred title of *Imperial*." The diploma, however, which was forwarded by Delambre, evinced somewhat of relaxation from this uncourtly rigidity of sentiment; for though the engraved document was headed, " *Institut de France*," the abbreviation *Imp.* was inserted, in a small hand, after the word " Institut."

But of all the tributes of respect which Dr Jenner received, that which was conferred by the University of Oxford, was one of the most gratifying; for that learned body, in the year 1813, unanimously voted to him in full convocation, by diploma, the degree of Doctor of Physic. It is to be regretted that the London College of Physicians did not follow so honourable an example, by opening their portals for his reception; and they thus, very inconsiderately, we venture to say, lost the opportunity of which the greatest bodies in Europe joyfully availed themselves, of enrolling Jenner's name in the list of their fellows. His exclusion, under such circumstances, must hardly be credited by learned foreigners, and is very much at variance with the habits and genius of the English nation.

The visit of the Allied Sovereigns in 1814, afforded to Dr Jenner an opportunity of becoming acquainted with the high estimation in which he was held by the most exalted personages; and the account of their personal interviews will be read with interest.

Some feelings of disappointment appear to have been entertained, that so auspicious a period should have been passed over, without its being attended with some public impress of consideration to Jenner. We do not quite understand of what description this was expected to be; but as far as it might refer to any personal dignity, we have every belief, that his wishes on this subject, had they been known, would have been met to the full extent which his position in society justified, both in respect to foreign orders, and marks of distinction at home. Public men are ordinarily too much occupied (and they were unquestionably so at that period) to look about for objects on which to bestow their regard and consideration. Vaccination was not likewise a new thing, and the application for Parliamentary recompense, (the only one which appears to have been requested,) had been complied with. But when we consider that our worthy and fortunate countryman Wylie, obtained a baronetcy from the English government, out of compliment to the Emperor of Russia, to whose army he was physician; and that the excellent and amiable Sir Alexander Crichton, Physician to the Emperor, received the honour of knighthood, we cannot doubt the force, and the readiness with which the principal potentates of Europe would have strengthened that disposition to do Jenner honour, which was possessed, in an eminent degree, by all classes of persons of this country, and for which the visit of those great personages would have afforded a very appropriate period. Jenner's name was known and respected all over the world; and there is an interesting address from the Chiefs of the five nations, assembled in council at Fort George, in Upper Canada, on the 8th of November 1807, in return for a present of his book. With this address, which was signed with their emblems, they transmitted "a belt and string of wampum, in token," say they, "of our acceptance of your precious gift; and we beseech the Great Spirit to take care of you in this world, and in the land of spirits." The influence which Jenner was capable of exercising with foreign states, was highly gratifying; and he had it in his power to employ it most worthily, in procuring the liberation from France of some friends who were among the detenus; and who, but for his interference, must have lingered out many additional years in a foreign land.

The powers of the continental governments were exercised much more effectually in diffusing vaccination, and in prohibiting inoculation for the small-pox, than could be done in a country like ours. But there is a curious anecdote given by Sir Alexander Crichton, in a letter to Dr Jenner, of the 12th of September 1812, which shows, that there are limits established by nature, even to the exercise of the most sovereign power. In this letter, he details the operations which had taken place in respect to cow-pox; gives the numbers vaccinated in the Russian empire as amounting to 1,235,597, and annexed an ukase from the Emperor, for rendering more efficient the previous regulations on the subject. In this ukase, it is enacted, that after three years, "there must not be found man, woman, or child, the newly born excepted, who have not been vaccinated;" but Sir Alexander adds, that "Notwithstanding the supreme order of His Imperial Majesty, that all his subjects be vaccinated within three years, we find, that powerful as His Majesty is, this cannot be executed. There is a power greater than sovereignty, namely, the conscience or religious opinions of men, and in one or two of the distant governments there exists a peculiar religious sect, belonging to the Greek church, who esteem it a damnable crime to encourage the propagation of any disease, or to employ any doctors, or to swallow any medicines under the visitations of God. Reason has been employed in vain with these poor people; they have been threatened with severe punishment in case they remained refractory on such points, but all to no purpose. They have no priesthood, but attempts have been made to gain those of the community who have most influence with them, but all to no purpose. You may well imagine that no punishment has been employed, though threatened; and the government has come to the wise resolution of leaving this dispute to time."—P. 186.

Dr Jenner remained three months in London during the summer of 1816, and then returned to Cheltenham; but in September of the following year, he had the severe misfortune of losing his wife, who had suffered very long from bad health, and to whom he was most warmly attached. Mrs Jenner was a lady of a very superior character; and it is to the well-regulated influence which she possessed over Dr Jenner, that Dr Baron attributes much of that freedom which he displayed from the taint of human passions and imperfections, when they were most likely to be excited; and a great deal of his modesty and forgiving temper. "It was to her devout and holy life, and her meek, and firm, and consistent conduct, that we are," Dr Baron feels convinced, "in some measure, enabled to dwell with so much pleasure upon the memory of her husband." From the time of her death, Dr Jenner lost much of his relish for public occupations, and he retired immediately after that event to Berkeley, from which he was seldom afterwards more than two or three days absent. The death of his much esteemed friend, Dr Parry of Bath, from apoplexy, took place in no long time after, and was a serious affliction to him; but his most poignant subsequent bereavement was the death of his only daughter, Mrs Bedford, who died in August 1822, soon after giving birth to a daughter. Dr Jenner had been long subject to giddiness in his head, and was seized with an attack of this kind in August 1820, while in the garden, and was supposed to have laid some time in a state of insensibility in consequence of it. He gradually recovered, but he became ever after remarkably sensible to external impressions, and particularly sounds of a certain description. He had no indications of paralysis, or serious affection of the brain, and in a few months resumed all his former occupations, continuing to chronicle his thoughts and opinions, according to his wonted habits, with much perseverance. It appears, indeed, that he had noted down some particulars relative to the case of Mr Joyner Ellis, a school-fellow, and an old friend, the very night before his own fatal seizure, or perhaps on the very morning of it.

He was apprised of Mr Ellis's death before he was up. He arose as usual, and came down to the library; but not appearing at breakfast, the servant went to ascertain the cause, and found him in a state of insensibility on the floor, and in complete apoplexy, with the right side paralyzed. He expired about three in the morning of the 26th of January 1823, having had stertorous breathing from the commencement, with complete general insensibility, and the pupils of the eyes contracted to a point, and unaffected by strong light. It does not appear that any examination took place after death,—a circumstance which, we think, is much to be regretted, considering the very prominent position which Dr Jenner occupied with the profession and the public all over the world; and bearing in mind, too, the important pathological incident which had occurred not many years before, on which, as well as the symptoms which were consequent on it, an examination was calculated to throw much light. It would have been in accordance, also, with the ardent spirit of investigation for which the deceased was so eminently distinguished, to have made his remains, like those of his illustrious friend, John Hunter, subservient, as far as examination would go, to public benefit. And we do not see how the prejudice against such kind of inquiries is to be successfully combatted in the world, if domestic feelings, however natural and even praiseworthy they may be, should, in the case of such men as Jenner and Parry, (for we do not understand that any examination took place in the case of the latter,) prevent that inspection being made, for which both of them would have been anxious, with their most valued patients, under such symptoms, had they been living. In the case of Dr Jenner, there was no wife and daughter to lament his loss,—deeply as every other relative and friend must have deplored it; and we earnestly wish, therefore, that Dr Baron could have made up his mind; and we well know the extent of the effort to press the measure on the family.

Sir Gilbert Blane was desirous of having the remains of Jenner entombed in Westminster Abbey; but this did not take place; and they were deposited in the chancel at Berkeley, by the side of his beloved wife, on Monday the 3d of February 1823; and the mournful ceremony was witnessed by a considerable number of his most particular and attached friends.

Till the very day preceding his death, Dr Jenner was employed, " with his usual ardour, in professional avocations, and in attending to those acts of mercy and benevolence that the wants of his neighbours and the severity of the season demanded." He had walked to a neighbouring village, to order supplies of fuel to some of the poor people, and in his way back called on his nephew, Stephen, in his painting-room. The latter was singing, during his work, a popular Scotch air, and Dr Jenner, who detected an inaccuracy in the tune, gave himself a stanza or two as a correction.

" Hardly, at any period of his life," says Dr Baron, " did the excellent and lamented individual, to whom his work has done so much justice, appear to be more alive to every moral and intellectual enjoyment, than immediately previous to the attack which so soon terminated his existence."

Dr Baron gives many interesting particulars relative to the important effects on population which vaccination has had over all Europe, but more especially in those countries where its adoption has been most general. He adverts to the occurrence of small-pox after vaccination; and states, that very different degrees of success have attended the practice of different individuals, arising, probably, either from varieties in the efficacy of the lymph, or differences in the progress of the affection. After very minute inquiry, Dr Baron informs us, that he has not been able to find out more than six or eight cases of small-pox after cow-pox among the whole of Dr Jenner's patients, though the number was exceedingly great. The experience of Gloucestershire does not countenance the idea, that the protecting power of vaccination is weakened by the lapse of years; for Dr Baron states, that those who were vaccinated thirty or forty years since appear to have resisted the small-pox contagion, just as much as if they had previously had that disease.

ART. VII.—*Vital Statistics of Glasgow.* I. *Statistics of Fever and Small-Pox prior to* 1837. II. *Statistics of Fever for* 1837. III. *Remarks suggested by the Mortality Bills.* By ROBERT COWAN, M. D., (One of the Physicians to the Glasgow Royal Infirmary. Read to the Statistical Society of Glasgow, 28th April 1837 and 17th May 1838.) 8vo. Glasgow, 1838. Pp. 54.

THIS little work does great credit to the talents, industry, and ingenuity of Dr Cowan. From its condensed nature, it is hardly possible to give an abstract of it. We shall therefore select the more important points ascertained by the author, recommending to our readers to lose no time in perusing the work for themselves.

The first paper is devoted to the statistics of fever in Glasgow, for the last forty-two years; the statistics of the Glasgow Fever Hospital for 31st October 1835, till 1st November 1836; and the statistics of small-pox in Glasgow.

1. *Statistics of Fever in Glasgow for the last forty-two years.*—Dr Cowan begins by giving a few remarks on the geographical situation, climate, progress of the population, and amount of hospital accommodation of Glasgow.

> "The city of Glasgow is situated in latitude 55 degrees 51 minutes 32 seconds north, and longitude 4 degrees 17 minutes 54 seconds west, according to the determination of Dr Wilson, formerly Professor of Astronomy in the University.
>
> "The mean heat of Glasgow was formerly determined by Dr Thomas Thomson to be 47 degrees 75 seconds. In the second edition of Dr Cleland's folio Statistical work, pp. 102 to 109, the yearly quantity of rain is given for thirty years, as ascertained by Dr Couper, Professor of Astronomy in the University, showing a yearly average of 22.328 inches. The least quantity in any one year was 14.468, in 1803, and the greatest 28.554, in 1828."

With the assistance of Mr John Couper and Professor Nichol, he has been enabled to bring down Dr Cleland's table to the end of 1836. In 1829 the quantity of rain which fell was 24.491 inches; in 1830, 25.923; in 1831, 22.937; in 1832, 22.785; in 1833, 19.908; in 1834, 21.861; in 1835, 22.666; and in 1836, 31.710.

The following table will enable the reader to compare the numbers admitted into the hospital, and the numbers affected with fever, with the population at the different dates.

Year,	1791.	1801.*	1811.*	1819.†	1821.*	1831.*
Population,	66,578	83,769	110,460	417,197	147,043	202,426

At the census of 1831, 3,908 was the amount of the rural, and 198,518 of the town population,

The increase in the population in Glasgow has arisen in a very great degree from immigration, chiefly of females, favoured by the increased demand for female domestics and for female labour, in the numerous factories and bleachfields in the neighbourhood of the city. The age of the immigrants is generally from 15 to 20, —a fact of some importance in reference to fever, as will be afterwards pointed out. Most of this class, from being emancipated at an early age from parental discipline, are more liable to disease. In 1819, there was one native of Ireland in 9.67 of the population; in 1831, one in 5.69. From this increase alone, without taking into account the influx of labourers from other quarters, it is obvious that the relative proportion of the middle and wealthy classes to the labouring has been yearly on the decrease, and hence one source of the increasing mortality in Glasgow.

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* Government census, † Dr Cleland.

"At the census of 1831, of 143,142 individuals, the amount of population between 10 and 70 years of age, the occupations of 103,001 were narrated, and of these 29,287 were either directly or indirectly connected with the manufacture of cotton goods. The number of labourers was about 6614, the number of paupers 5006.

"A large proportion of the inmates of our hospital are drawn from the labouring class of the community, from the hand-loom weavers, from the females employed in manufactories, and from the class of paupers, while comparatively few males, above the class of labourers, employed in the public works, apply for admission.

"The following tabular view of the amount of population, and rate of mortality, for the last fourteen years, is extracted from a letter addressed by Henry Paul, Esq. a member of this society, to the Lord Provost, &c. &c. on the subject of the Mortality Bill for 1835. By comparing this table with one to be afterwards given of the annual number of fever patients treated in the hospital, the influence of fever on the amount of mortality will be readily ascertained.

Years.	*Population.*	*Burials.*	*Rate of Mortality.*
1822,	151,440	3690	1 : 41.00
1823,	156,170	4647	1 : 33.75
1824,	161,120	4670	1 : 34.50
1825,	166,280	4898	1 : 33.94
1826,	171,660	4538	1 : 37.82
1827,	177,280	5136	1 : 34.51
1828,	183,150	5942	1 : 30.82
1829,	189,270	5452	1 : 34.71
1830,	195,650	5785	1 : 37.73
1831,	202,420	6547	1 : 30.91
1832,	209,230	10,278	1 : 20.35
1833,	216,450	6632	1 : 32.63
1834,	223,940	6728	1 : 33.28
1835,	231,800	7849[*]	1 : 29.53
Mean mortality from 1821 to 1835 inclusive,			1 : 33.24
1836,	244,000	9143	1 : 26.687[†]

The following view is given of the nature of the hospital accommodation in Glasgow.

"The Royal Infirmary, for the reception of medical and surgical patients, was opened in the month of December 1794, and contained accommodation for about one hundred and fifty patients. An addition was made to it in 1816, containing 80 beds. One-half of the fever hospital was opened in 1829, and the other in 1832, and, with some additional accommodation afforded since, can now receive two hundred and twenty patients.

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[*] In the burials from 1822 till 1835, there were included 6257 still-born.

[†] Of the 9143 burials in 1836, there were 702 still-born.

" The permanent hospital accommodation was—
From 1795 till 1816, - 150 beds.
1816 till 1829, - 230
1829 till 1832, - 330
1832, - - 450
At which it still remains."—P. 7.

Besides this permanent accommodation, it has been found necessary, on many occasions, to provide temporary hospitals, and to use apartments in the Infirmary never intended for the purpose. This, with the exception of the time when cholera prevailed, has been owing solely to the prevalence of typhus.

" In 1818, a temporary fever hospital was erected at Spring Gardens by public subscription, fitted to contain 200 patients. It was opened on the 30th March 1818, and closed on the 12th July 1819.

" This hospital was again opened in 1827, at the expense of the Infirmary, and kept open for five months.

" In 1828, a temporary booth was erected on the Infirmary grounds, capable of containing 68 patients.

" A fever hospital, with 135 beds, was opened at Mile-End, on the 9th January 1832, and closed the same year."—P. 7.

This accommodation, however, for fever has, on various occasions, been found insufficient, and many applicants have been necessarily refused admission, and thus the disease has spread in their respective neighbourhoods. The following tables show the enormous increase of this disease within late years.

" Table of the total number of Patients treated in the Glasgow Royal Infirmary, from 1795 till 1836, distinguishing the number of fever patients each year.

Year.	Tot.	Fev.	Year.	Tot.	Fev.	Year.	Tot.	Fev.
1795	226	18	1802	729	104	1809	886	76
1796	338	43	1803	806	85	1810	935	82
1797	545	83	1804	678	97	1811	826	45
1798	569	45	1805	719	99	1812	877	16
1799	631	128	1806	700	75	1813	1022	35
1800	733	104	1807	726	25	1814	1135	90
1801	702	63	1808	840	27	1815	1340	230
1. Period.	3744	484	2. Period.	5198	512	3. Period.	7022	574

Year.	Tot.	Fev.	Year.	Tot.	Fev.	Year.	Tot.	Fev.
1816	1511	399	1823	1759	269	1830	2010	729
1817	1886	714	1824	2091	523	1831	3183	1657
1818	2289	1371	1825	2438	897	1832	2974	1589
1819	1861	630	1826	2317	926	1833	3082	1288
1820	1670	289	1827	2725	1084	1834	3879	2003
1821	1454	234	1828	3133	1511	1835	3260	1359
1822	1596	229	1829	2321	865	1836	5130	3125
4. Period.	12167	3866	5· Period.	16784	6075	6. Period.	23,518	11,750

For the last 3 or 4 years, patients with small-pox and scarlet fever have been included in the returns of fever.

" Table, exhibiting the number of Patients admitted into the Temporary Fever Hospitals at Spring Gardens and Mile-End :—

Spring Gardens, 1818–19, . .	1929
Mile-End, 1832, . .	1145
	3074

" The Patients admitted into the Hospital at Spring Gardens in 1827 were treated at the expense of the Infirmary, and are included in the number of Patients in the Infirmary return for that year.

" In the first Septennial period, the Fever Patients treated in the Infirmary were— 12.92 per cent. of the whole.

In the second,	9.84	—	—
In the third,	8.17	—	—
In the fourth,	31.77	—	—
In the fifth,	36.19	—	—
In the sixth,	49.96	—	—

and if to this table, strictly applicable to the Royal Infirmary, we add the numbers treated in the temporary hospitals, we will raise the per centage in the fourth period

From 31.77 to 47.62; and in the sixth period,
From 49.96 to 54.83.

" During the first 35 years embraced in the table, the number of patients affected with fever treated in the Infirmary amounts to 11,511, while in the last seven years it amounts to 11,751.

" Table, exhibiting the number of cases of fever treated by the district surgeons, from the 1st August 1827, till 1st August 1832, and in the years 1833–34–35–36, distinguishing the number sent to the Infirmary.

Year.	Number of Cases.	Sent to Infirmary.	Treated at Home.
1827–28	1281	281	1000
1828–29	1730	390	1340
1829–30	485	135	350
1830–31	898	306	592
1831–32	1428	336	1092
1833	681	294	387

Year.	Number of Cases.	Sent to Infirmary.	Treated at Home.
1834	936	538	398
1835	542	215	327
1836	1359	643	716
Total,	9340	3138	6202*

Respecting the patients in the third table, the following explanations are necessary. All the patients were treated at the public expense, and may be considered as puapers, as, before being attended by the district surgeons, a certificate is required from the elder of the district, stating that they are unable to pay for medicines and advice. The salaries of the surgeons, which are miserably low, amounting only to L. 21 each, and the medicines prescribed, are paid out of the poor-rates. All the patients thus attended reside within the burgh, the population of which in 1831 was 89,847, of which 12,554 were Irish, while the suburbs contained 202,426, of which 23,000 were Irish. No effectual measures have yet been adopted to place the poor of the suburbs in respect to medical attendance on a footing similar to that of the burgh, though the necessity of so doing is perfectly evident.

From the table it appears, that 33.5 per cent. of those attended by the district surgeons, are at a more or less advanced period of the disease, sent to the hospital, the remainder being treated at home. It would be of consequence to ascertain the comparative mortality of those treated in their own houses, and those sent to hospitals. But unless the sex and age of both classes be given, no satisfactory results can be obtained. Few of the district surgeons escape fever.

The following passage shows the difference between the amount of fever in Glasgow, and in other towns in Great Britain.

> " Manchester, with a population at the last census of 227,808, and which, in its constitution and density must nearly resemble that of Glasgow, has been for years, and is now comparatively free from fever. The average annual number treated in the Manchester fever hospital for seven years, ending in 1836, was . 497
> The annual average in Glasgow during the same period, 1842
> The number treated in Manchester hospital in 1836, 780
> The . . Glasgow . . . 3125

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* " The above Table shows the arduous and dangerous duties imposed on the district surgeons from fever alone. Few of these gentlemen escape an attack of fever. The salary allowed to each is L. 21 per annum, a sum quite inadequate for the duty performed; and, notwithstanding the eagerness with which, from professional ardour, the situation is sought for, the public ought, in justice to themselves and to the medical profession, to insist upon a more liberal remuneration being made to the district surgeons.'

Fever is now diminishing in Manchester, while it is increasing in Glasgow.

"The prevalence of fever in Glasgow, when compared with Manchester, is still more strikingly contrasted by the great change which has taken place in this respect. From 1797 to 1806, both inclusive, the number of the fever patients treated in the Glasgow Infirmary was only 883, while those treated in the Manchester fever hospital amounted to 4618.

"In Leeds, too, another manufacturing city, with a population at the last census of 123,393, the number of patients affected with fever and treated in hospital, amounts to 1923 during the last seven years, giving an annual average of only 274.

"In Newcastle and Gateshead, with a population of 57,917, the number of patients treated in the institution for the cure and prevention of contagious fever during the last seven years, amounts to 276, or 39 annually.

"In Liverpool, with a population of 189,242, 1700 cases of fever were treated in the hospital during 1836; but many of these belonged to the seamen of the port, a numerous class of the population.

"A comparative view of the state of fever in other towns in England, contrasted with that of Glasgow, would, I am afraid, only place the insalubrity of our city, as far as fever is concerned, in a more prominent and alarming point of view.

"In Edinburgh, with a population of 162,156, the number of fever patients admitted into the Royal Infirmary, for the last three years, has been as follows:—

Year ending 30th September, 1834,	712
. . . 1835,	900
. . . 1836,	658
	2270

giving an average of 756⅔ per annum."

Dr Cowan complains justly of the want of accommodation, and recommends that, instead of wasting the public money on temporary buildings, small unpretending edifices, capable of containing 50 or 60 patients each, should be erected in those situations where fever is most prevalent.

From what has now been mentioned, it is evident that since 1816, fever has been steadily increasing in Glasgow, and that those attacked with it constitute within a fraction of 55 out of every 100 patients treated in the hospital. It becomes a matter of consequence, therefore, to ascertain the cause. Dr Cowan thus expresses his views of the subject.

"This increase, especially during the last seven years, has taken place, not in years of famine or distress, but during a period of unexampled prosperity—a period when the wages of labour have been ample—the prices of provisions comparatively low, and every individual, able and willing to work, secure of steady and remunerating employment.

" True, indeed, the weather has not been favourable since 1830; and certain atmospherical phenomena inimical to health have existed, as may be inferred from the prevalence of various epidemics, though not appreciable by scientific instruments ; but these general causes have not acted so severely on other cities of the empire as they have upon Glasgow, which has numbered more victims from influenza, Cholera, and fever, in proportion to its population, than any other city in Britain.

" Many of the causes of the production and propagation of fever must be ascribed to the habits of our population ; to the total want of cleanliness among the lower orders of the community ; to the absence of ventilation in the more densely peopled districts ; and to the accumulation, for weeks or months together, of filth of every description in our public and private dunghills ; to the over-crowded state of the lodging-houses resorted to by the lowest classes ; and to many other circumstances unnecessary to mention."—Pp.12, 13.

For all these Dr Cowan suggests proper remedies, and enforces his views in a manner calculated to make a strong impression on those interested in the matter. Dr Cowan next presents us with a statistical account of the Glasgow Fever Hospital from 31st October 1835, till the 1st November 1836.

The hospital can accommodate, without being over-crowded, 220 patients. During the period referred to, 2655 patients were admitted, of whom 142 were treated by the clinical physicians, and 2513 by Dr Cowan. The following table exhibits the numbers admitted each month.

" 1835—	November . . .	124
	December, . .	140
1836—	January, . . .	141
	February, . .	125
	March, . . .	176
	April, . . .	203
	May, . .	246
	June, . .	272
	July, . . .	264
	August, . . .	306
	September, . .	303
	October, . ,	355
		2655."

The admissions thus in the first three months were 405, in the second 504, in the third 782, and in the fourth 964 ; in the first six months 909, in the last 1746. The number of patients admitted in 1835 amounted only to 1359, and on an average of the last eight years to 1477, while in 1836, the number was 3125, being greater than that of those treated in 1832, in the Infirmary (1589), and Mile-End Hospital (1145) 2734.

The quantity of rain was much above the average, and the temperature of almost every month lower than that of the previous year. The average residence of each patient in the Hospital was eighteen days. Of the 2513 patients treated by Dr Cowan, the males and females were nearly equal in number, being 1255 and 1258. The Scotch were 67.76 per cent. of the total admissions; the Irish 30.12; the English, &c. 2.10.

Though the hospital was intended strictly for fever,—cases of small-pox, scarlet fever, measles, erysipelas, and other diseases obtained admission, from mistake or otherwise. The following table exhibits the diseases treated during the year in the Fever Hospital, distinguishing the sex and nation of the patients, and the rate of mortality.

	Males.					Females.				
	Scotch.	English.	Irish.	Total.	Deaths.	Scotch.	English.	Irish.	Total.	Deaths.
Fever,	712	34	370	11	*	782	14	345	1141	*
Small-pox,	45		4	49	13	46			46	13
Scarlet-fever,	15		5	20	2	35	1	5	41	5
Measles,	3			3	2	1		1	2	
Erysipelas,	3			3	2	1		1	2	
Inflammation, brain,	1		1	2	2	1		1	2	2
Delirium tremens,	3			3	2					
Chronic abscess, brain,			1	1	1					
Quinsy,	5			5						
Catarrh,	7	1	3	11	1	4		1	5	
Pleurisy,	3		4	7	3	3			3	3
Inflammation of lungs,	14	2	11	27	8	6		2	8	2
———— bowels,						1	1	1	3	2
Consumption,	2			2	2	3			3	3
Rheumatism,	2			2	1	1			1	
Diseased liver,						1			1	1
Old age, vagrancy, &c.	3		1	4						
	818	37	400	1255		885	16	357	1258	

Of the fever patients, the Scotch form 66.10, the English 2.12, and the Irish 31.67. Of 95 patients affected with small-pox, 91 were Scotch, and 4 Irish; and all were, with two or three exceptions, above twenty years of age. Of 61 patients labouring under scarlet fever, 50 were Scotch, 10 Irish, and 1 English.

According to the Government census of 1831, the population of Glasgow was 202,426, of whom 93,724 were males, and

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* The mortality of the fever patients will be given at page 536 and 537.

108,702 females. The number of Scotch was 163,600, of English 2919, of Irish 35,554, and of foreigners 353.

The proportion of Irish treated in the Fever Hospital is much less than what has generally been believed.

The following table shows the number of persons of different ages in Glasgow, and the suburban parishes of Barony and Gorbals, in 1831.

Under 5.	5 to 10	10 to 15	15 to 20	20 to 30	30 to 40	40 to 50	50 to 60	60 to 70	70 to 80	80 to 90	90 to 100	100 and upw	Total.
Ms. 15422	13127	10491	8489	15177	12179	8685	5549	3228	1090	260	26	1	93724
Fs. 14855	12580	10720	12256	23008	14240	9329	6099	3692	1502	385	32	4	10870
Tot. 30277	25707	21211	20745	38185	26419	18014	11648	6920	2592	645	58	5	20242

Dr Cowan next gives three elaborate tables, the first exhibiting the ages of the male fever patients, distinguishing the number of admissions at each age for each month; the second, a similar one for the females; and the third showing the number of patients at each age, and classing them according to distinct periods.

> "From an examination of these tables, it appears that the period of life at which Fever is most liable to occur, is from the age of 20 to 25 years for the males, when the proportion is 21.23 per cent., and from the age of 15 to 20 for females, when the proportion is 23.83 per cent.
>
> "The number of females at every age prior to 25 exceeds that of the males.
>
> "The number of females admitted under 20 years of age is 571
> Males, ... 480
>
> Excess of females under 20, ... 91
>
> or 8.64 per cent.
>
> "This fact must be kept in view, as it has an important bearing on the relative mortality of the sexes.
>
> "The number of both sexes admitted from the age of 20 to that of 30 years, is 1216, or 53.87 per cent.
>
> "The number admitted under the age of 30 years, is 1766, or 78.24 per cent.
>
> "The admissions rapidly diminish after the age of 40. Of 2257 individuals affected with fever, 2075 were under 40 years of age, and only 182 above it."—P. 201.

In respect to the rate of mortality in the Fever Hospital, Dr Cowan's observations lead him to the following results. In males the mortality is 14.83 per cent., in females 8.92. The deaths within the first twenty-four hours amount to 17 per cent. in the males, and 9 in the females. At almost every period of life embraced in the observations, the mortality among the male fever patients was greater than among the female. At the age of fifteen, the mortality is very nearly the same in both. At thirty, the mortality of the males is more than double that of the females. It is greatest in females at the age of forty-five. Under twenty years of age, the mortality of the males was 6.04 per cent., of the females 4.90; of both sexes under thirty, 8.35 per cent., and above thirty, 24.84. After the age of ten, the mortality from fever slowly increases, till the age of thirty-five. From 2.63 per cent. at ten, it gradually rises to 14.92 at thirty-five; at forty, it is 22.36, and at fifty, 39.06. The mortality among the Scotch and Irish was precisely the same, while among the English, if any inference can be drawn from the small numbers, it was considerably less.

From discussions which took place in the Medical Society of Glasgow, Dr Cowan was led to pay particular attention to the prevalence of the exanthematous eruption in fever,—the *eruption typhoide* of the French, and the following are the results of this investigation.

"TABLE of the Patients admitted with Fever, distinguishing Males from Females, and the numbers of each sex in whom the Typhoid Eruption appeared.

	MALES.		FEMALES.	
1835.	Eruption.	None.	Eruption.	None.
November,	27	14	25	33
December,	14	16	19	30
1836.				
January,	31	12	29	25
February,	22	21	19	24
March,	36	24	29	34
April,	68	26	52	32
May,	84	29	77	27
June,	90	20	105	21
July,	104	11	96	21
August,	115	39	108	19
September,	116	24	120	29
October,	143	30	140	27
	850	266	819	322

Of 2257 patients—850 males and 819 females = 1669 had eruption
And 266 do. and 322 do. = 580 had none."

"TABLE of the per centage of patients with Typhoid Eruption, each Quarter, distinguishing Males from Females.

	Males.	Females.
1st Quarter,	63.15	45.34
2d do.	63.95	52.63

	Males.	Females
3d do.	82.24	80.11
4th do.	80.08	83.06
	76.16	71.77

Total—2257 : 1669 = 73.99.

"From these Tables it appears that the proportion with eruption varied each month; and that, with the exception of the quarter ending in October, the number of females with eruption was always less than that of the males.

"For the first six months of my attendance, less than one-half of the females had eruption, while in the last six months four-fifths had it; and, upon the average of the whole year, 71 out of every 100.

"Of the males the monthly proportion also varied considerably, but on an average of the first and second quarters, 63 per cent. had the typhoid eruption.

"During the last six months, the proportion of males and females in which the typhoid eruption appeared was nearly the same. At the close of the year, in 76.16 per cent. of the males, and 71.77 of the females, the typhoid eruption had occurred, giving as an average of the whole cases 73.99 out of every 100 admitted.

"From these tables I am warranted in the inference, that the exanthematous eruption is not an essential character of the fever of this country, as during the first six months it occurred in only 49 per cent. of the females, and 63 per cent. of the males; and, besides this, even in an epidemic, in which it is a distinguishing feature, it is not invariably present, as during the last six months it was absent in nearly 1–5th of those admitted."—Pp.25.26.

Dr Cowan concludes this part of his observations, by declaring his belief in the contagious nature of fever, and mentions the following facts in corroboration of his opinion, which is founded on other considerations. Forty-seven per cent. of the whole patients admitted into the Fever Hospital, either ascribed the origin of their disease to contagion, or had been exposed to its influence. All the clerks in the Fever Hospital, for many years past, have been seized with fever, unless they had had it previously to their election. During the last year, twenty-seven of the nurses caught the disease, and five of them died. Several of the students have been affected, and the apothecary died of it. His own immunity he ascribes, either to his being past the period of life at which fever usually occurs, or to the attacks which he had in the early part of his career.

The next subject to which Dr Cowan directs his attention, is the Statistics of Small-Pox. The introduction of inoculation, though it diminished the relative, he conceives to have increased the absolute mortality of the disease, since by this practice the disease, which before its introduction, occurred only epidemically, at long intervals, was kept continually inactive, and thereby produced a rate of mortality which will now be credited by those alone who inspect the registers. Though adopted by the intelligent classes, it was rejected by the less informed, and hence, as shown by the bills of mortality, the number of deaths from small-pox increased after its introduction. The Glasgow bills of mortality furnish no data from which it can be demonstrated, that mortality from small-pox was greater at the close than at the commencement of the eighteenth century; but in London and other towns the fact has been ascertained.

" The following table exhibits the total deaths under ten years of age, and the deaths under ten from small-pox in Glasgow, for thirty years, divided into three equal periods; and is compiled from tables prepared from the registers of this city, by the late Dr Robert Watt, and published in the Appendix to his work on Chincough.

Year.	Total.	Small-Pox.	Year.	Total.	Small-Pox.	Year.	Total.	Small Pox.
1783	719	155	1793	1126	389	1803	940	194
1784	877	425	1794	759	235	1804	863	213
1785	744	218	1795	1048	402	1805	884	56
1786	941	348	1796	797	177	1806	786	28
1787	1016	410	1797	884	354	1807	899	97
1788	1059	399	1798	864	309	1808	1775	51
1789	1058	366	1799	1105	370	1809	1187	159
1890	1236	336	1800	746	257	1810	1027	28
1791	1367	607	1801	766	245	1811	1274	109
1792	902	202	1802	985	156	1812	1278	78
1st period.	9919	3466	2d period.	9080	2894	3d period.	10913	1013

" The ravages of small-pox were never before more vividly illustrated than in the foregoing table.

In the first period of ten years, the total deaths under ten years of age amounted to 9919, and the deaths from small-pox, to 3466, being 35.94 per cent. and rather more than one-third of the whole deaths under ten.

" In the second period, the total deaths under ten are 9080, and the deaths from small-pox 2894, or 31.87 per cent; and in the last period, the total deaths under ten are 20,913, and the deaths from small-pox 1013, or only 9.28 per cent.

" I am not aware that small-pox was so fatal in any town as it appears to have been in Glasgow. In Berlin, the deaths from small-pox were for a short time, as 1 in 4, but more generally as 1 in 7, of the whole deaths under ten years of age, while in the city and suburbs of Glasgow, it was fatal in the proportion of one in three of the deaths under ten years, and that not for one or two years merely, but for a long period.

"The great saving of human life is rendered apparent from the third period embraced in the table. Up to the very moment of small-pox inoculation being superseded by cow-pox the mortality is immense, and the instant the latter is employed, the mortality becomes trifling in comparison."—P. 27, 28.

Although from 1812, till 1838, no statement was made of the annual number of deaths from small-pox, Dr Cowan, from the increased rate of mortality of late years, and the period of life in which it has occurred, has been for a long time of opinion, that the disease has prevailed to a greater extent than has been generally supposed.

"This must remain a matter of conjecture for the period prior to 1835, unless some person, possessing the persevering industry of Dr Watt, shall attempt the task of completing his tables from 1812 till 1835.

"The following table gives the deaths from small-pox, according to the mortality bills for 1835-36, but does not include the whole of them, as the causes of death were not ascertained in many instances; and at the Tollcross burying ground, in which the interments, in 1835-36, amounted to 645, the diseases have not been recorded.

Table of the deaths from small-pox, 1835-36.

	Under 1 year.	1 to 2.	2 to 5.	5 to 10.	Above 10.	
1835,	204	154	75	17	23	=473
1836,	202	174	144	23	34	=577
	406	328	219	40	57	=1050

"The annual average number of deaths under ten years of age, for ten years prior to 1812, from small-pox, was 101, while, during the two last years, they have amounted to 903.

TABLE of the number of patients with small-pox, annually admitted into the Glasgow Royal Infirmary, from 1795 till 1836.

Year.	No.	Year.	No	Year.	No	Year.	No.	Year.	No.	Year.	No.
1795	4	1802		1809	10	1816	14	1823	46	1830	10
1796	2	1803		1810	2	1817	7	1824	37	1831	12
1797	4	1804	1	1811		1818	11	1825	3	1832	3
1798	9	1805	3	1812	4	1819	7	1826	1	1833	14
1799		1806	1	1813	2	1820		1827	25	1834	62
1800	1	1807	5	1814	2	1821	33	1828	4	1835	72
1801	1	1808		1815	4	1822	5	1829	1	1836	110
	21		10		24		77		117		283

"This table shows a progressive increase in the number of patients with small-pox admitted into the Infirmary for some years past, and also how small a proportion of the adult population was admitted during the years in which small-pox was carrying off hundreds annually under ten years of age.

"Of the 110 patients treated in the Infirmary in 1836, ninety-five came under my charge; and the next table gives the sex, nation and district of each patient, with the proportion vaccinated, and the number of deaths. The patients, with three exceptions, were adults.

	Highlanders.	Lowlanders.	Irish.	Total.
Males,	34	11	4	49
Females,	36	10	0	46
	70	21	4	95
Males Vaccinated,				32
Females,	17	6	0	23
Males Unvaccinated,				17
Females,	19	4	0	23
Males Dead,	9	3	1	13
Females,	10	3	0	13

"The first remarkable feature in the above table is, that out of 95 patients affected with small-pox, there are only four natives of Ireland.

"The second is, that of 91 natives of Scotland, 70 are Highlanders, and 21 natives of the Lowlands. A very large proportion of the Highlanders were from the remote islands, and all of them, without a single exception, had recently arrived in Glasgow.

"Fifty-five of the patients had apparently been vaccinated, and forty never had this operation performed. Nearly one-half of the Highlanders had marks on their arms, but these were not in general the result of what I would consider perfect vaccination.

"No death occurred in any individual who presented the appearance of having been properly vaccinated.

"The Irishman who died was a vagrant, who had not been vaccinated, and who was exposed to the contagion of small-pox in a lodging-house at Finnieston."—Pp. 29, 30.

Dr Cowan observes, that the occurrence of so many as ninety-five cases of small-pox would, at first sight, induce us to believe that the efficacy of vaccination is not so great as has been anticipated, but that farther investigation will show the table last quoted affords a powerful recommendation, if any were wanted, to the practice. The Irish, as has been already stated, furnish 30 per cent. of the admissions into the Fever Hospital, while of ninety-five individuals attacked with small-pox, four only were of that country. The cause of this immunity will, according to the author, be found in the general practice of vaccination among the lower classes by the surgeons of the county and other dispensaries of Ireland. To the neglect of vaccination, and to the practice of it with impure lymph, deteriorated, perhaps, in the transmission, must be ascribed the prevalence of small-pox among the Highlanders. The true cause of the alarming mortality of the disease in Glasgow, Dr Cowan affirms to be the neglect of vaccination, and not the occurrence of small-pox after vaccination. The remedy for this is obvious.

"The increasing rate of mortality in the city of Glasgow has given rise to much speculation as to its causes, and many opinions have been promulgated regarding it.

"It has been ascribed solely to the prevalence of fever; but this cause has acted chiefly on the adult proportion of the population, and consequently has been limited in the extent of its operation. Small-pox has had its share in augmenting the mortality; but, unlike fever, it exerts its influence during infancy: and to it, in a great measure, must be attributed the increased mortality under ten years of age.

The deaths from fever, in 1835, were 412
1836, 841
from small-pox, in 1835, 473
1836, 577
2303

Of the deaths from fever, 186 were under ten years of age,
And of those from small-pox, 993

"The following table exhibits the proportion which the total deaths, and the deaths under ten years of age, bear to the population at different periods, and the per centage which the deaths under ten years are of the total deaths.

Years.	Rate of Mortality.	Rate of Mortality under 10 years.	Deaths under 10 years of age.
1821,	1 in 39.89	1 in 75.29	50.27
1831,	1 in 30.91	1 in 60.04	51.48
1835,	1 in 29.53	1 in 49.92	59.15
1838,	1 in 26.68	1 in 48.07	55.50

"The above table presents but a melancholy index of the state of public health since 1821, and shows how severely, during the last two years, the augmented mortality has affected the earlier years of life."—P. 32.

2. *Statistics of Fever in Glasgow for* 1837.—It has already been shown, that fever has increased in Glasgow during the seven years prior to 1837, during a period of unexampled prosperity. From the close of 1836 a depressed state of trade, secret combinations leading to *strikes*, which deprived many of all adequate means of support; the high price of coal, diminishing the hours, and consequently the wages of labour, and placing fuel beyond the reach of the lower classes for domestic purposes, and the increased price of grain, have been so many causes in operation to increase the amount of disease.

"The fever, however, was chiefly, nay, almost wholly confined to the labouring classes, and to the districts which they inhabited, while among the wealthy and middle classes of society, it was comparatively seldom met with, and when it did occur, was not spread by contagion through all the inmates of the family, as was usually the case among the families of the poor, but was confined to a single individual. It is thus evident, that the concentrated virulence of the epidemic fell exclusively upon a portion only of the community."—P. 34.

The following extracts give a condensed view of the number and mortality of fever cases during this year of distress, as well as some important conclusions drawn from the facts collected by the author.

"The number of fever patients treated in hospital in 1837, was	5387
in 1836,	3125
Being an increase in 1837 of . .	2262
The number of fever patients treated by the district surgeons in their own houses, in 1837, was . .	2320
in 1836, . .	716
Being an increase of	1604

"The above table gives a very inadequate idea of the comparative frequency of fever in 1836 and 1837. During 1836, till the month of December, every applicant for admission was received into the hospital; while in 1837, seldom a day passed without numerous applicants being refused admission for want of room, and many were deterred from applying for admission from a knowledge of the overcrowded state of the wards. Besides, the number of poor treated in their own houses in the *suburbs*, cannot be ascertained.

"A reference to the mortality bills will express more forcibly the comparative frequency and mortality of fever during the last three years, than any statement deduced from the hospital returns.

"TABLE of the Deaths from Fever recorded in the years 1835, 1836, and 1837.

1835,	412,	being 1 to every	15.571	of whole deaths, & 1 to every	570.388 of populat·
1836,	841,	.	10.036	. . .	290.130
1837,	2180,	.	4.711	. .	116.055

"The rate of mortality from fever during 1837 has been calculated as high as 1 in 10 of those affected; and upon this supposition 21,800 individuals have been attacked with the disease. The usual average mortality from fever treated in hospitals, is 1 in 15; but the intensity of the epidemic, arising from the poverty and destitution of the population, carried the rate of mortality higher than ever before known in Glasgow.

"Upon the assumption that the rate of mortality from fever was 1 in 15 of those attaked in 1835, 1 in 12 in 1836, and 1 in 10 in 1837, which calculations will be found nearly correct, the number of individuals who have been affected with fever in Glasgow during the last three years will be as follows:—

In 1835,	. .	6,180
1836,	. .	10,092
1837,	. .	21,800
		38,072

"TABLE of the Deaths from Fever each month, in the years 1836 and 1837, distinguishing the Males and Females, and giving the monthly proportion which the deaths from Fever bear to the whole number of ascertained deaths.

Months.	1836.			1837.			1836.	1837.
	Males.	Females.	Total.	Males.	Females.	Total.	Proportion to whol deaths.	
January,	31	14	45	108	93	201	1 in 17.55	1 in 9.81
February,	16	11	27	77	61	138	24.74	7.1
March,	33	24	57	124	100	224	11.82	4.20
April,	27	37	64	114	88	202	10.00	4.0
May,	39	28	67	120	113	233	10.29	3.22
June,	38	33	71	109	90	199	8.91	3.34
July,	31	30	61	106	88	194	11.14	3.67
August,	51	31	82	101	71	172	8.89	4.9
September,	32	24	56	73	53	126	11.46	5.11
October,	52	37	89	74	75	149	7.71	3.7
November,	42	47	89	83	64	147	8.86	4.2
December,	73	60	133	98	97	195	6.15	3.73
	465	376	841	1187	993	2180		
1st Quarter,	80	49	129	309	254	561		
2d Quarter,	104	98	202	343	291	634		
3d Quarter,	114	85	199	280	212	492		
4th Quarter,	167	144	311	255	236	491		
	465	376	841	1187	993	2180		

"Many interesting observations may be drawn from this table. It shows the slow progress of an epidemic disease when trade is prosperous, compared with what occurs in seasons of distress. Up to November 1836, the period at which the commercial embarrassments were felt, the mortality from fever had not been rapidly increasing. In November it was just about double what it had been in January preceding, the number of deaths being 45 in January, and 89 in November.

"The moment, however, the effects of the stagnation in trade extended to the working classes, the mortality increased with fearful rapidity, aided no doubt by the season of the year, the high price of grain, and the scarcity or high price of fuel. The deaths from fever in the four months preceding 1st December, 1836, were 316; for the four months following, 696.

"The table also marks the period at which the epidemic reached its maximum amount of mortality, viz. in the second quarter of 1837, and in the month of May in that quarter, being the month succeeding that in which the strike of the cotton spinners took place, by which 8000 individuals were thrown out of employment.

"The establishment of soup-kitchens, and the provision of work for the unemployed operatives in the months of May, June, and July, must have materially aided in arresting the progress of the epidemic.

"Many expect that fever is to subside suddenly, but an examination of the above table, and of the mortality bill for 1838, will prove the fallacy of such opinions, by exhibiting a gradual diminution in the numbers attacked.

"The table also exhibits the relative proportion of male and female deaths from fever in 1836 and 1837, to which has been added a similar statement for 1835.

"During the last three years the deaths of males from fever have been 54.47 per cent., and of females 45.52 per cent. of the whole deaths from fever. While the mortality of the males from fever has in each year exceeded that of the females, it has done so in a variable proportion, as will be seeen from the following table.

"TABLE of the per-centage of the deaths of Males and Females from Fever.

	Males.	Females.
1835,	52.93	47.08
1836,	55.29	44.70
1837,	54.44	45.55

"The above table and the mortality bills confirm the opinion formerly given from the statistics of the fever hospital, that the mortality from fever at every period of life is greater in males than females."—Pp. 37—40.

Dr Cowan concludes this section with some very judicious remarks and useful suggestions, respecting the means to be taken to provide suitable accommodation for the poor affected with fever. These we are compelled at present to pass over.

In the remarks suggested to the author by the mortality bills, the reader will find some facts strongly corroborative of certain principles established by M. Quetelet, in reference to the effects of disease, poverty, &c. increasing the number of deaths, and diminishing that of marriages and births.

We close this short view of the leading facts contained in Dr Cowan's work, by recommending it to all who take an interest in vital statistics, and not only to them, but to medical men in general, and all authorities to whom the health and police of the country are interested. The author deserves great praise for the careful and judicious collection and arrangement of a most useful body of facts, as well as for the cautious and scientific conclusions which he has deduced from them.

ART. VIII.—*On the Influence of the Seasons on the Mortality at different ages in Belgium.* By A. QUETELET. Brussels, 1838. Pp. 42 and 5 plates.

THIS memoir, originally read before the Academy of Sciences of the French Institute in 1835, and of which an analysis was given in the memoirs of that body, is now reprinted in its present form, with considerable additions. Statistical researches on the comparative mortality of the human race, have long attracted the attention of scientific men; and the value of these researches have been duly appreciated by the application of their results to practical purposes. M. Quetelet is well known as a zealous cultivator of this branch of science; and the present memoir, we have no doubt, will be considered a valuable accession to medical statistics.

The author commences his memoir with stating very shortly the present state of our knowledge on the subject; and the modifying effects of sex, town or country residence, and of the seasons, on mortality. To this last head. the author's attention is chiefly directed in the present paper. The tables have been drawn up from the official statistical documents of Belgium, and embrace about 400,000 observations; they include all Belgium, and extend for a period of five years, viz. from 1827 to 1831.

1. *Of the Mortality during the different Months.*—The following Table, showing the influence of Age and that of the Seasons on mortality, is drawn up without reference to sex or locality.

Ages.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
From 0 to 1 mon.	1.39	1.28	1.21	1.02	0.93	0.83	0.78	0.79	0.86	0.91	0.93	1.07
1 to 3	1.39	1.18	1.15	0.95	0.89	0.82	0.83	0.94	0.83	0.92	0.97	1.13
3 to 6	1.24	1.06	1.02	0.90	0.95	0.95	0.99	1.06	0.99	0.94	0.86	1.02
6 to 12	1.28	1.21	1.27	1.18	1 06	0.84	0.76	0.87	0.81	0.82	0.86	1.03
12 to 18	1.10	1.11	1.24	1.30	1.25	1.03	0.88	0.81	0.74	0.77	0.78	0.98
18 to 24	1.23	1.18	1.21	1.18	1.03	0.84	0.80	0.76	0.75	0.81	1.01	1.18
2 to 3 yrs.	1.22	1.13	1.36	1.27	1.12	0.94	0.82	0.73	0.76	0.78	0.91	1.01
3 to 5	1.23	1.16	1.26	1.29	1.13	0.94	0.78	0.74	0.73	0.79	0.89	1·05
5 to 8	1.20	1.17	1.32	1.24	1.20	0.96	0.78	0.74	0.76	0.75	0.85	1.02
8 to 12	1.08	1.06	1.27	1.34	1.21	0.99	0.88	0.82	0.81	0.76	0.80	0.96
12 to 16	0.95	0.95	1.14	1.14	1.19	1.04	0.97	0.95	0.96	0.81	0.86	1.04
16 to 20	0.93	0.94	1.07	1.18	1.15	1.03	1.00	0.99	0.89	0.87	0.95	1.01
20 to 25	0.97	1.00	1.09	1.02	1.09	0.96	0.90	0.92	0.96	0.95	1.03	1.11
25 to 30	1.05	1.04	1.11	1.06	1.02	1.02	0.91	0.96	0.95	0.93	0.97	0.97
30 to 40	1.11	1.13	1.11	1.04	0.99	0.92	0.85	0.94	0.99	0.95	0.94	1.03
40 to 50	1.17	1.15	1.13	1.05	0.99	0.86	0.86	0.94	0.93	0.87	0.95	1.11
50 to 65	1.30	1.22	1.11	1.02	0.93	0.85	0.77	0.85	0.89	0.90	1.00	1.15
65 to 75	1.43	1.32	1.18	0.99	0.91	0.77	0.71	0.80	0.88	0.86	0.98	1.17
75 to 90	1.47	1.39	1.16	1.01	0.87	0.77	0.67	0.75	0.84	0.84	1.00	1.21
90 & upwards.	1.58	1.48	1.25	0.96	0.84	0.75	0.64	0.66	0.76	0.74	1.03	1.29
Mean	1.26	1.20	1.17	1.08	1.00	0.88	0.80	0.81	0.86	0.86	0.94	1.09

This table shows that the influence of the seasons is far from being the same for different ages, although the winter season is generally more unfavourable than that of summer. Human life is divided into many different periods, in order that the results may be better perceived. The mean mortality is represented by unity, and regard is also had to the unequal lengths of the months.

This table also points out very clearly, that, for the first year of existence, there are two periods when the number of deaths attain their maximum, the one very distinctly marked in January and February, the other much less so, in August. The one may be named the *absolute*, the other the *secondary* maximum. This maximum of mortality for the summer period is not apparent during the first month of existence, but becomes very marked during the middle of the first year. The two minimum periods of mortality, which for the new-born are united in August, separate more and more till the sixth month, when they are found, the one in April and the other in November. From this period they again approach each other, and by the end of the first year of existence they are both united in the month of September.

The next table given by the author is one showing the mortality of the new-born of either sex in the different months, and also the proportions in town and country. In regard to this table, it may be remarked, that the average mortality of the male sex appears to be from a fifth to a third greater than that of the female sex, and that the general mortality of both sexes is greatest in the months of January and February. This table does not give the data requisite for comparing the comparative mortality of town and country.

As the age advances from *one* to *twelve*, it is remarked that the absolute maximum of mortality goes by a succession of oscillations from January to May; keeps for some short period in this month; and then retrogrades till about the thirtieth year of life, when the maximum of mortality is again found in February, and continues fixed in this month till the latest period of life.

The absolute minimum of mortality, after the first year, is found to occur pretty regularly five or six months after the maximum, and this falls in August and September, from the ages of one to eight; in October from the ages of eight to twenty; and fixes itself in July for the remainder of life.

The *secondary maximum* of mortality, after the first year, is not again noticed till towards the age of twelve, when it is found in the month of December, and continues there till about the twentieth year. After the twenty-fifth year of age, it leaves the month of December, and for the rest of life, is found to occur permanently during the months of August and September.

The author gives a table with the view of it showing this, and also the relative values of the maxima and minima of mortality at all ages, by which it appears, that the rigors of winter cause a mortality amongst those above sixty-five years of age, from a third to a half greater than amongst infants of the most tender age. In order to comprehend this at a glance, he has constructed a table, which he gives at the end of the Memoir, where a series of curved lines, by their rise above, or their fall below a straight line representing the mean mortality, indicates the ratio of mortality for the different periods of life, during the different months of the year.

The maxima and minima of mortality for either sex, taken separately, fall in almost every instance on the identical months as those mentioned above, which were constructed for both sexes taken together; but the absolute number of deaths of either sex at different ages varies considerably,—so that it is the influence of sex, rather than of season, which causes more of one sex to die at one period than another.

An interesting table is given to prove this statement. It represents the relative mortality of the two sexes,—the male deaths being reckoned as unity.

TABLE showing the relative Mortality of both Sexes, the number of Male deaths being taken for unity.

Months.	One Month.	1 to 2 Years.	12 to 16 Years.	16 to 20 Years.	20 to 25 Years.	40 to 50 Years.	90 Years upwards.
January,	0.75	0.95	1.32	1.04	0.83	1.21	1.18
February,	0.70	0.91	1.42	1.08	0.83	1.22	1.30
March,	0.79	0.90	1.11	1.17	0.78	1.18	1.50
April,	0.73	0.94	1.23	1.18	0.80	1.21	1.44
May,	0.75	0.96	1.45	0.97	0.80	1.30	1.40
June,	0.67	0.97	1.28	1.16	0.73	1.18	1.20
July,	0.70	1.00	1.32	1.08	0.78	1.17	1.42
August,	0.79	0.92	1.20	0.98	0.77	1.08	1.03
September,	0.79	0.98	1.31	1.01	0.73	1.06	1.47
October,	0.67	0.99	1.22	1.01	0.68	1.11	1.50
November,	0.76	1.05	1.20	0.99	0.64	1.11	1.08
December,	0.76	1.05	1.20	9.96	0.64	1.18	1.48

From this it appears, that, during the whole of the first year of life, there dies more of the male sex than of the female, and that the average relative mortality is nearly the same during the whole of this period. The number of deaths are nearly equal during the second year; but the mortality amongst the females is greater between the ages of twelve and twenty. The mortality for the male sex is again greater between the ages of twenty and twenty-five; and for the rest of life, the mortality is again greatest amongst the females.

The author's next table shows the relative proportion of deaths for town and country.

TABLE showing the Mortality in Town and Country.

AGES.	Proportion of Deaths among Females, for one Death among Males.	
	Town.	Country.
Still-born.	0.75	0.59
From 0 to 1 mo.	0.75	0.73
1 to 2	0.73	0.84
2 to 3	0.82	0.83
3 to 6	0.79	0.86
6 to 12	0.94	0.97
1 to 2 yrs.	0.94	1.03
2 to 5	1.00	1.09
5 to 14	1.12	1.07
14 to 18	1.22	1.34
From 18 to 21 yrs.	1.02	1.08
21 to 26	0.79	0.90
26 to 30	1.00	1.17
30 to 40	1.14	1.60
40 to 50	0.98	1.20
50 to 60	0.93	0.85
60 to 70	1.04	0.95
70 to 80	1.30	1.00
80 to 100	1.47	1.09

The next point discussed is the proportion of still-born children in town and country, and what influence the seasons have on the proportion of deaths in this way. The conclusion with regard to the last of these inquiries is, that the seasons have very little effect on the numbers of still-born children, there being merely a very slight increase of the numbers during the months of January, February, and March. The proportions for town and country are, that of 3354 still-born children, 1341 occurred in the towns, and 2013 in the country.

The epochs of the maximum and minimum mortality in town and country appear to coincide in their general results, only that they are more distinctly marked in the country than in the town.

The second part of the Memoir is directed to the consideration of *the state of the atmosphere during the different months of the year.* This we can notice but very generally, and must refer to the paper itself for the details.

The author gives tables of the diurnal and annual variations of the thermometer and barometer, for a series of years. The hygrometric state of the atmosphere for a similar period is also stated. In one table is given the number of days when it rained, or snowed, was fair, thundered, clear, or cloudy, &c. and the last table connected with this portion of the subject is one of the prevalent winds for each month.

The third part of the Memoir contains *the comparison of the meteorological, with the mortality tables for the different months;* and in order to facilitate the study of this subject, the author has given three tables where one curved line again represents the mortality during the different months, and others the temperature, barometrical pressure, and hygrometric state during the same period. This comparison is very interesting, for we are thus enabled to perceive the strong coincidence which he has proved to exist between the increase of mortality and increase of cold. The slight increase of mortality which occurs immediately after the great heats of summer, may be attributed chiefly to the sudden vicissitudes of the atmosphere at that period of the year.

The next table is one which shows the proportional mortality of the different months in the Belgic provinces; from the years 1816 to 1825, and from 1833 to 1836. We shall content ourselves with giving the summary of this same table drawn up by the author, showing the proportional maximum and minimum number of deaths, and the months in which these two epochs occur.

TABLE showing the maximum and minimum of mortality.

Provinces.	Value.		Difference.	Epochs.	
	Maximum.	Minimum.		Maximum.	Minimum.
Luxembourg,	1.41	0.65	0.76	March,	August.
Namur, -	1.29	0.77	0.52	February,	Do.
Limbourg, -	1.26	0.77	0.49	March,	Do.
Hainault, -	1.26	0.79	0.47	February,	Do.
Liége, -	1.21	0.81	0.40	March,	Do.
West Flanders,	1.22	0.83	0.39	Do.	Do.
East Flanders,	1.19	0.82	0.37	February,	July.
Brabant, -	1.19	0.83	0.36	March,	Do.
Anvers, -	1.13	0.81	0.32	February,	Do.

From this it appears that the province of Luxembourg is that in which the influence of the seasons is most felt, the mortality of the month of March being nearly double that of August. The neighbouring provinces, as Namur, Limbourg, Liége, and Hainault still mark strongly the influence of the seasons on the mortality; but this is less marked in Brabant and the two Flanders; and in the province of Anvers the mortality has attained its minimum. The province of Luxembourg is situated in the most elevated part of the kingdom, is the most exposed to the vicissitudes of the seasons, and is the coldest of them all. On the other hand, the provinces where the mortality is least, are those whose situation is lower, and where the temperature seems to be more equable.

It cannot be believed that the mortality is increased in proportion to the absolute degree of cold, for the northern climates do not bear out such an assertion; but it may at least be affirmed, that man becomes accommodated to the climate he inhabits, and that, once accustomed to the mean temperature of that climate, he suffers more by excess of cold than by excess of heat in that latitude.

In examining these tables of mortality for the Belgic provinces, it is observed, that there is a slight increase of deaths at the end of summer; this secondary maximum of mortality, as the author terms it, is observed more particularly in the provinces of Anvers and Eastern Flanders, and seems to be attributable to the intermittent fevers which prevail in these provinces. This secondary maximum is still to be remarked in Brabant and Limbourg, at the termination of the summer season, but is scarcely to be observed in the provinces of Luxembourg, Namur, or Liége.

The conclusions which the author deduces from these facts are as follow:—

1. In studying the influence of the seasons, or the mortality in Belgium, the life of man presents two marked periods; the one the period of physical developement, which extends to the age of twenty-five years; the other embraces the rest of life.

2. After the physical developement is completed, the most unfavourable season for man is winter; and the other seasons follow in the order of spring, autumn, and summer.

The absolute maximum of deaths takes place in February, and the minimum in July. The difference which exists between their values goes on increasing till the end of life; at twenty-five years of age it is only as 125 to 100, and ends by becoming as 255 to 100.

3. To man arrived at his full developement, there exists an apparently well marked connection between the rise and fall of the thermometer, and his mortality, and yet, after the warmest month, which is also the one when fewest deaths occur, there is a sensible increase of mortality. The month of October, which follows this increase, shows a minimum of deaths relative to the month preceding, or that following.

4. Taking man *during his developement*, and not taking into account the first year which follows his birth, during which period the infant is in some sense identified with the mother who suckles it, we find that his chances of mortality are divided. The minimum of deaths still occur in July, and the maximum at the coldest period after the month of January. The increase of the mortality which follows the excess of the summer heats is also greater for children than for adults.

This increase of mortality which follows the excess of the summer heats and the winter colds, and which cuts off the infant during the first year of his life, had been formerly noticed with regard to the first three months of existence, by MM. Villermé and Milne Edwards. Nevertheless, the action of summer is but little felt during the first months of existence, and only arrives at its maximum about the sixth month.

5. From the first to the twelfth year of life, the maximum of mortality leaves January, and by a succession of oscillations, gradually arrives at May, where it remains for some time; it then retrogrades from the sixteenth to the twenty-fifth year, and remains stationary in February till the close of life.

The minimum of deaths after the first year, with pretty considerable regularity, occurs five or six months after the maximum period,—occurring in August from the first year till the eighth. From eight years of age till twenty, it is met with in October, where, as we have already stated, it continues to form a relative minimum to the end of life.

6. During the developement of man, but after the first year, the minimum of deaths does not occur in July.

When we place the seasons according to their relative mortality, they stand in the following order, spring, winter, summer, autumn. If we consider the period of puberty only, they arrange themselves somewhat different, viz. spring, summer, winter, and autumn; whilst for adult man their order is, winter, spring, autumn, summer.

7. From 12 to 25 years of age, a relative minimum of deaths occurs in January, which, for other ages, is the month in which most deaths occur.

8. In taking into consideration the distinction of sexes, we find that, for the different epochs of life, the maxima and minima numbers, both absolute and relative, fall on the same months for each sex, and that the relations of the maxima and minima numbers for each sex are nearly the same.

9. But there exists a great difference when we compare the absolute number of male deaths to those of the female for each period of life. Thus, immediately after birth, for every four deaths amongst the males, there occur only three amongst the females. This difference, so unfavourable to the males, diminishes successively till towards the age of 2 years, when the number of deaths of either sex is nearly equal, and continues so till the age of 12. From 12 to 20 there are more female deaths than males; the contrary, however, occurs from 20 to 25. From 25 to 30 the deaths of either sex are equal. From 30 to 50 more women than men die; from 50 to 65 the contrary happens. But after 65 the deaths amongst the females are numerically greater than amongst the males.

From this it follows that the deaths in the two sexes are equal from the first to the twelfth year of age, from the 25th to the 30th, and again towards the 65th year. The deaths amongst the males are more numerous after birth, from 20 to 25 years of age, and from 50 to 65; they are less numerous, on the other hand, than the deaths amongst the females from the 12th to the 20th year, from 30 to 50, and after 65 years of age.

10. The influence of season and sex on the still-born is nearly the same as for those born alive, although less marked.

11. A town or country residence produces no essential difference in the epochs of the maximum and minimum mortality, which is not accounted for by the seasons; but the differences be-

tween the maximum and minimum are more strongly marked in the country.

ART. IX.—*The Surgical Anatomy of the Perinæum.* By THOMAS MORTON, formerly one of the House-Surgeons of the University College Hospital. Illustrated with Lithographic Plates and Wood Engravings. London, 1838. Royal 8vo. Pp. 80.

THE immediate object of this work is to enable the student to make the best use of his dissections of the perinæal region, by placing in his hands plain and comprehensive rules, by which each structure, as it appears under the knife, may be easily displayed.

The author, who has evidently had considerable experience in teaching practical anatomy, seems fully sensible of the advantages that are to be derived by the student, from a combination of graphic illustrations with the descriptions, and has introduced several correct, and very beautiful delineations, which must materially assist the dissector, in acquiring an accurate and intimate knowledge of the relative anatomy of the numerous and important parts contained within the limits of this region of the human body. The description of each stage of the dissection is accompanied with appropriate surgical observations, concerning the changes which are induced in the parts by disease, as illustrative of the operative procedures which are performed in this region. By adopting this method, the tediousness of a mere anatomical description is very much diminished, at the same time that the attention of the student is especially directed to the parts of greatest consequence, and his memory greatly assisted by associating the study of anatomy with that of surgical pathology. The author's style is easy and lucid, without being diffuse.

In the section which is devoted to the consideration of the developement of the perinæum, those malformations which seem to be owing to an arrest of the process of formation are explained in a manner likely to be easily intelligible to the student. Several chapters are added, in which the principal operations performed on this part of the body are fully described, and accompanied with such observations as the experience of the author has enabled him to make.

Of the plates and wood-cuts which illustrate the work, it is sufficient to say that they are anatomically correct, clear, and beautifully executed. We have no hesitation in recommending this publication as likely to prove useful to students actually engaged in the schools, and to those who, having entered practice, may find occasion to keep up or refresh their recollections of the anatomy of the perinæum.

ART. X.—*Medical Portrait Gallery. Biographical Memoirs of the most Celebrated Physicians, Surgeons, &c. &c. who have contributed to the advancement of Medical Science.* By THOMAS JOSEPH PETTIGREW, F.R.S., F.S.A., F.L.S., &c. Imperial Octavo. London and Paris, 1838, 1839.

THE desire to know something of men who have risen to distinction in any of the departments of literature or science, or the liberal arts, is so universal, that biographical information is always interesting, and may be made, at the same time, highly instructive. In the several divisions of the art of healing, it is interesting to contemplate the gradual and steady rise of genius under adverse circumstances, the slow but final elevation of merit to its just position, and to know the means by which talent, perseverance, and good conduct worked their way amidst multiplied impediments to that pre-eminence, in which past difficulties are remembered with pleasure, and numerous circumstances, at first sight indifferent or adverse, have been made to concur as it were, to the final advantage and prosperity of the individual.

To this class of publications belongs the present work. It consists of biographical sketches of individuals who have been, or at present are, distinguished as physicians or surgeons, anatomists, physiologists, or pathologists.

Biography may be attempted in two modes; either in giving memoirs of the lives and writings of the illustrious dead, or in recording the history of the progress and services of eminent persons still living. The former is certainly the safest undertaking, because in treating of the character and merits of those long dead, there is less risk of partiality, and less inducement to employ the tone of adulation or the language of flattery. As the ancients wisely observed, that no man could be said to be happy till he was dead; so it may be with equal truth asserted that it is impossible to form a very just opinion of any one till after death. These remarks are particularly applicable where it becomes necessary to examine critically the scientific or literary labours of the individual whose life is the subject of narrative.

In some instances, however, biographical writers have departed from this prudent method, and have undertaken to record the lives and transactions of persons still living. It is evident that this is an undertaking at once of great delicacy and arduousness. There are perhaps few individuals whose lives can bear to be written, or their characters delineated during life, with perfect impartiality and complete regard to historical accuracy. As no character is absolutely perfect, it is difficult to say how far a strict narrative of simple facts, however useful to readers, will be agreeable to the subject or his friends; and it may happen, that in the desire to delineate a pleasing and perfect picture of a living person of eminence, less attention is paid to historical fidelity than the public benefit demands. "Simul veritas," says Tacitus, "pluribus modis infracta; primum inscitia respublicæ ut alienæ, mox libidine assentandi aut rursus odio adversus dominantes. Ita neutris cura posteritatis, inter infensos del obnoxios. Sed ambitionem scriptoris facile adverseris: obtrectatio et livor pronis auribus accipiuntur."

Though the attempt to describe living characters must be therefore allowed to be liable to several objections, still a certain proportion of history of this description may perhaps be usefully and innocently brought before the eye of the public. If the rule "de mortuis nil nisi bonum," be imperative on the principle of Christian charity and good will to the departed, it is not less forcibly so in the case of the living; and it may therefore be admitted, that biographical sketches, even of living characters, may be given in which, like the panegyrics of the ancient Romans, and the eulogies of the modern French, all the favourable points are prominently brought into view.

Upon these principles, in a great degree, the present work has been composed. Though it contains accounts of many who have at different periods of the history of medicine been distinguished as benefactors of the art of healing, and who have long ago disappeared from the scene, it contains accounts of a number who are at present before the public, and who, it is to be hoped, will long continue to benefit by their services the world at large, and improve and encourage by their example their younger professional brethren.

The twelve numbers now published complete two volumes. The first of these contains biographical sketches and memoirs of Æsculapius, Albinus, Sir Henry Halford, Ruysch, Haller, Sir Anthony Carlisle, Linacre, Akenside, Sir Charles Mansfield Clarke, John Caius or Kaye, Morgagni, Dr Blundell, Radcliffe, Bichat, Sir A. Cooper, Dr Copland, Dr Cooke, and Dr Mead.

The second volume contains Dr William Hunter, Dr Jenner, Dr Baron, Dr Baillie, Dr Bright, Sir B. Brodie, Mr John Hunter, Mr Lawrence, Malpighi, Sir John Pringle, Dr Clutterbuck, Dr Huxham, Mr Wilson, Dr James Wardrope, Vesalius, Mr Abernethy, and Dr James Johnson.

The whole amount of sketches, therefore, is thirty-five, of which thirteen are memoirs of living characters.

In the memoirs of those who are no longer before the world, Mr Pettigrew has shown considerable judgment in selecting the most important particulars, and placing them prominently before the reader. Novelty it would be unreasonable here to expect. We may say, however, that in his account of the life of Akenside, are detailed several curious particulars which are not found elsewhere.

In the memoirs and notices of living physicians and surgeons, the author has shown much judgment, as well as taste and good feeling, in the manner in which he has stated the facts, delineated the characters, and enumerated the writings of the different individuals whose lives he has sketched. In this duty he has confined himself chiefly to the professional or public life of the individual, and avoided the mere personal memorials.

These sketches will be perused with considerable interest; and the work itself is calculated, on the one hand, to make known the characters of physicians and surgeons to their brethren, and the public at large, and to invest, with a just degree of dignity and interest, the characters of the members of the profession. The duties of the medical practitioner, though always laborious, anxious, and important, are often performed in situations so obscure, and in circumstances so unfavourable to the acquisition of celebrity, that the world at large take in them much less interest than in those of any of the other liberal professions; and hence it often happens that little is known of individuals who have risen to great and deserved eminence, and whatever is known is confined to mere professional readers. The sketches in the present work may be useful in procuring for the members of the medical profession, a greater degree of attention than that which has been hitherto bestowed on them, and also of procuring it for them at a period of life when it will be more advantageous than it usally is.

In conclusion, the work of Mr Pettigrew is an undertaking which deserves the patronage of the profession, and of the public. The engravings are very beautifully executed.

We regret to perceive by the observations in the concluding number, that the reception of the work has not hitherto been such as to induce the publisher to continue it; and the author has therefore determined to continue it on his own account, though with some modifications and changes. As it has been said that the memoirs are too short, in order to obviate this objection, the author proposes, in the subsequent numbers, to devote the quantity of letter-press assigned to three to two memoirs, and also to introduce wood-cut figures of new instruments or other objects of interest.

We believe it is impossible to produce a work which shall please all. But we think it important, while every reasonable suggestion for improvement is adopted, to deviate as little as possible from the plan originally contemplated, and so far carried into effect. At one time of the publication, it appeared to us, that the chronological order might be the most expedient and useful. But it must be allowed that the present has the advantage of an agreeable diversity; and by mingling the biographical memoirs of modern days with those more remote, gives a greater degree of interest in perusal.

We hope, whatever may take place, that the author will persevere in his undertaking, because we are sure it will increase in interest as it advances.

ART. XI.—*Illustrations of Osteology*, By THEODORE S. G. BOISRAGON, M. D. Cheltenham. London, 1839. Folio. Three Plates, Lithographed.

THESE lithographic engravings, executed in imitation of Italian drawings in black and white chalk, upon tinted paper, form the first and the larger part of a work intended to illustrate the anatomical peculiarities of the bones of the human body.

The first drawing gives a very correct view of the bones of the right hand and foot, articulated,—the palm of the former, and the sole of the latter being presented to the eye of the observer. To render these views complete it would have been requisite to give representations of the dorsal surface of both parts.

The second drawing contains views of the first rib, the third rib, and one false rib, the clavicle, *scapula, humerus, radius, and ulna*, —all also seen on one side only. In all the prominent characters they are represented with great beauty and fidelity.

In the third drawing are given views of the *femur, tibia*, and *fibula* in the same manner, and with equal happiness of execution.

The objection, that only one view of these objects is given, is admitted by the author, and accounted for, if not extenuated by the circumstance, that he made them from the original objects in the Museum of the University of Edinburgh, at a time when he did not propose to publish them, and when he took them chiefly for his own use. He promises, however, that, should the present attempt receive adequate encouragement, he may give other views of the same bones, and also views of some other bones not included in the present list.

Upon this it may be observed, that the best mode of securing a favourable reception to his work is by making it as complete as possible; and as one view of any bone can never give a complete view, however accurate it may be as far as it goes, the wisest course is to give at least two good views of each.

The figures are very correctly drawn, and very ably represented and shaded; and we would recommend the work to all students, and others who wish to study the configuration of the bones with attention.

The shaded drawings are accompanied with outline figures and explanatory references.

The whole work is to consist of five plates, the two last of which will contain views of the *Cranium, Vertebrae*, and *Pelvis*.

PART III.

MEDICAL INTELLIGENCE.

PATHOLOGY.

Researches on Suppuration. By George Gulliver, Esq. Assistant Surgeon to the Royal Regiment of Horse Guards.—(*Phil. Magaz.* S. 3, Vol. xiii. No. 81, Sept. 1838.)—In this paper, Mr Gulliver adduces various facts to prove that, in inflammatory and suppurative fevers, it is possible to detect globules of purulent matter in the blood; and he consequently infers that the presence of these purulent globules is the cause of the peculiar symptoms observed in cases of this class,—in short, the proximate cause of the sympathetic inflammatory, sympathetic typhoid, and hectic fevers.

The mode of examination was partly chemical, partly microscopical. As water acts rapidly and energetically on the blood-globules, dissolving them so as to render them invisible, while the globules of purulent matter undergo no change after being long kept in water; so while the blood-globules are diffused through the fluid, the globules of purulent matter fall to the bottom, where they may be easily seen, and their characters determined by means of a good microscope. Ammonia also instantly renders the blood-globules invisible, while it acts slowly on the purulent globule; and acetic acid acts on the blood-globule and that of purulent matter in a manner equally characteristic. The microscope used for the purpose now specified was that with the deep object glass of Mr Ross, already mentioned in the paper of Dr Davy. To distinguish the globules of chyle from those of purulent matter, Mr Gulliver observes, that it is necessary to remember that the medium diameter of purulent globules is $\frac{1}{2800}$ths of an inch, which is above twice that of those of chyle.

In prosecuting this inquiry Mr Gulliver employed evidence of two sorts. He first produced inflammation artificially in one or more of the tissues in brute animals, and he then recognized the presence of globules of purulent matter in the blood of one or other of the large veins. Thus in the blood obtained from the right ventricle of the heart of a dog, in which acute inflammation was produced by injecting a weak solution of corrosive sublimate into the cellular tissue of the thigh, in the blood of the *vena cava* of a dog, in which both *tibiæ* and the cellular membrane were inflamed, in experiments on necrosis, and in the blood of the inferior *cava* of a dog destroyed by *peritonitis*, artificially induced, globules of purulent matter were seen by the microscope.

He also injected purulent matter into the *pleura* of one dog, into the *peritoneum* of another, and into the crural veins of other two dogs; and in the first two cases, and in the last, he detected the presence of globules of purulent matter in the blood after death.

The second kind of evidence employed by Mr Gulliver is that obtained from submitting to examination the blood of persons destroyed by various diseases, in which purulent matter is formed in the course of the disorder, as small-pox, *peritonitis*, suppurative inflammation of the leg, with inflammation of the vein, pleurisy, diffuse inflammation of the thigh, and pulmonary consumption; and in the whole of these cases he found globules of purulent fluid in the blood of the chambers of the heart or large veins after death.

The opinion originally founded on the microscopical observations of Mr Hunter, Sir Everard Home, and M. Bauer, that the globules of purulent matter are merely globules of blood modified by the inflammatory process, as advanced by Sir A. Cooper, Dr Young, and M. Gendrin, Mr Gulliver seems disposed, in some degree, to consider probable. He does not, however, agree with the latter author in regarding purulent matter as a modification of fibrin; and he adds that, in regard to the Batrachian reptiles, he never could induce suppuration in their tissues after repeated trials; and he consequently does not agree with M. Gendrin in admitting the facility of observing the transformation of blood-globules into globules of purulent matter.

He thinks that the matter often found in the centre of clots of fibrine is not purulent, but mere softened fibrine, which, though resembling purulent matter in some particulars, differs from it in chemical and microscopical characters.

His notion of the nature of the process of suppuration is ingenious. He thinks that the experiments performed by him render it probable that it is a sort of proximate analysis of the blood. As the effused fibrine produces swelling, or is attracted to the contiguous tissue for the reparation of injury, the blood globules altered by stagnation become useless, and are discharged in the shape of purulent matter, as waste from the system. Suppuration, therefore, he thinks, may be a physiological rather than a pathological phenomenon, purulent matter being an excrementitious discharge,—a part of the blood which has become effete and noxious during the reparative process, whether this process may have been employed in limiting the extent of an abscess, or in healing breaches of continuity. If, however, purulent matter be formed in the capillaries, in consequence of the stagnation and coagulation of their contents, this matter, he thinks, might be mixed in large quantities with the blood in cases where no declared or manifest suppuration has taken place

Regarding the observation of Muller, that the smaller capillaries have only the diameter of a blood globule, he promises on a future occasion to show, from the result of experiments, that these vessels become sufficiently enlarged during inflammation to contain a row of purulent globules.

To the remark, that purulent matter is often formed without any obvious addition of fibrine to the neighbouring parts, he answers, that this is not a healthy but a diseased form of suppuration; that in this kind of suppuration, the fibrine is broken down, mixed, and excreted with the purulent matter, and hence are formed the flaky curdy masses so often associated with this form of suppuration.

He also infers that the presence of purulent matter in the blood is the cause of the symptoms of irritation and fever generally observed in diseases in which inflammation proceeds to suppuration.

On the Effects which result from the introduction of Pins into the Digestive Organs. By M. Ollivier, d'Angers. (*Annales d'Hygiene Publique*, Jan. 1839.)—Rose Melanie Selter was tried at the Court of Assizes for the attempted homicide of a child two months and a-half old, by forcing it to swallow pins. After the pins had been swallowed, the child was seized with fits of suffocation so severe as to cause serious alarm for its life. This was on the 7th of April; and for several successive days it manifested considerable uneasiness, and appeared to have some obstacle in its throat, which prevented it swallowing freely. However, on the 10th April, the pains ceased, and the infant began to recover its health. The cause of its sufferings was not known at this time; but on the morning of the 11th April three pins were passed by stool; and on the evening of the same day four others; and next morning two more, making in all nine pins. After this the child recovered its health, and did not appear to have been injured by the pins passing through it.

M. Ollivier was led, from being consulted in this case, to examine into the published cases where needles or pins had been swallowed, with the view of ascertaining the amount of danger likely to result from the introduction of such bodies into the digestive organs. We shall notice shortly the result of his inquiries.

Numerous cases are on record where needles and pins have been swallowed, from which it appears that, in many instances, they cause no appreciable inconvenience or injury, but, in other instances, give rise to symptoms more or less alarming,—depending on whether the pin has penetrated the coats of the pharynx or œsophagus, or transfixed the cartilages of the larynx or trachea, or the coats of the stomach. A pin may remain fixed in the coats of the stomach without its presence causing much uneasiness. M. Ollivier saw a pin bent on itself, traversing a fold of the mucous membrane of the stomach, in the body of a patient who had fallen a victim to the operation of lithotomy. In this case there was only slight thickening, with induration of the coats where they were pierced by the pin. Another case is related where a brass pin, about fifteen or sixteen lines in length, merely caused slight uneasiness in the part supposed to be occupied by that body, so long as it remained in the intestines; but all unpleasant symptoms disappeared when the pin was expelled.

In certain cases the needles and pins which have been swallowed make their appearance under the skin, in different regions of the body, giving rise to the formation of small abscesses, on opening which the pins or needles may be extracted. Cases of this kind are on record, where hundreds of pins have been swallowed, and yet have not caused death, the patients having died of other diseases. Dr Silvy, (Mem. de la Soc. Med. d'Emulation, Vol. v. p. 181,) in a maniacal case which he attended, made out the existence of 1400 pins in various parts of the muscles of the body and limbs, many of which he extracted during life. This person died of *phthisis pulmonalis.* A very singular circumstance was, that none were found in the lungs.—A case is also mentioned, which occurred under the care of Dr Villars, (Dict. des Sciences Medicales, Tom. vii. p. 66,) where more than 800 needles and pins were extracted from the various parts of the body. The patient was a young girl, who for twelve days was in a state of delirium, during which time she had swallowed these needles and pins. They appeared over the whole surface of the trunk and of the limbs, and were successively extracted. She recovered.

These bodies are not, however, always thus harmless when introduced into the body; for cases are recorded where serious diseases, and even death, have been produced by them. Arnaud and Saviard (Journal de Savans, Nov. 1791,) found large pins in the testicle; they had found their way into this organ, and caused in it the developement of carcinomatous degeneration. Schenck, (Obs. Med. Chir. lib. 3, Obs. 10,) relates a case where a needle which was swallowed pierced the coats of the stomach and the liver, and caused death. Bayle (Nouvelles de la Republique des Lettres, Jan. 1795, Art. 5,) relates the case of a man who complained, for a long period, of an acute pain in the hypogastric region, where a collection of purulent matter formed. On the abscess being opened, a very large quantity of fetid purulent matter escaped, and the discharge continued for months. At last the patient died, worn out by the excessive discharge; and it was found, on dissection, that the abscess extended to the ureter; the coats of this tube were ulcerated, and a pin was found transfixing the thickness of all the coats. Dupuytren, (Traité des Blessures par armes de guerre, Tom. i. p. 82,) gives the case of a maniacal woman, who fell a victim to the numerous abscesses which formed over the surface of all the body, and in all of which needles or pins which she had swallowed, were found. M. Guersaut has also related to the author a case, where a fatal result followed the swallowing of a needle. A child was seized with vomiting, which continued obstinately for several weeks, and, from the symptoms which accompanied it, it was feared that softening of the stomach had taken place. The child died after two months of constant suffering; and, on dissection, a needle was found at a little distance from the pylorus, traversing the coats of the stomach, and fixed pretty deeply in the substance of the liver. No inflammation existed round this foreign body, which had evidently been the cause of the sufferings and death of the child.

M. Ollivier in conclusion remarks, that these last cases are very rare, yet they are sufficient to prove that death sometimes results from the introduction of these bodies into the digestive organs, though such a result is to be looked on as the exception, not the general rule.

MATERIA MEDICA AND THERAPEUTICS.

Bi-meconate of Morphia. By P. Squire, Esq. (*London Medical Gazette*, March 9th 1839.) Mr P. Squire, impressed with the idea that the combination of the active principle of opium, as prepared by Nature, would prove more beneficial as a therapeutic agent than the artificial salts, instituted a number of experiments, with the view of procuring the bi-meconate of morphia as free of the other ingredients of opium as possible. He has at length succeeded in obtaining a tolerably pure salt, which, from the trials which have been made of it, has fully answered his expectations, as to its superior medicinal power over the other preparations of opium. This salt is given in solution, made nearly of the strength of the common laudanum.

Dr Macleod, who, at the request of Mr Squire, made trial of this salt, in his letter to that gentleman thus expresses himself: "It appears to me a very mild and efficient preparation, rarely producing headach or other discomfort. It has repeatedly answered, in the most satisfactory manner, where opium had disagreed, and has succeeded in some cases where the other salts of morphia (the acetate and hydrochlorate) had failed to give relief."

Mr Henry Brandon details, in a letter to Mr Squire, his own case, in which the superiority of this preparation is evidenced. He says, "I have been a martyr to a spasmodic affection of the muscles for upwards of fourteen years, and was obliged, after trying every other means in vain, to have recourse to opium. I have taken crude opium, laudanum, the aqueous extract of opium, black-drop, *liquor opii sedativus*, and the acetate of morphia, and I declare that not one of them has succeeded so well as the meconate of morphia, which relieves much sooner, and without disturbing the stomach, leaving the system altogether in a more natural state of repose."

Dr A. T. Thomson, in a letter to Mr Squire, speaks also very highly of this salt of morphia. He says, "I have given a fair trial of your new preparation of opium, the bi-meconate of morphia, which, when separated from many of the other constituents of opium, undoubtedly possesses anodyne properties superior to any of the other salts of morphia in ordinary use."— "I have administered it in three cases in private practice, well calculated to illustrate its properties.

"The first was a neuralgic pain of the left side of the face, extending from the temple to the molar teeth of the upper jaw. It remitted during the day, but returned with frightful severity during the night." Calomel and extract of opium, hydrochlorate and acetate of morphia, black drop, belladonna, extract and tincture of hyoscyamus, extract of aconite, disulphate of quina, and arsenical solution were all successively tried without relief. "The strength of the patient was nearly worn out from want of sleep, and under these circumstances your solution was prescribed. Thirty minims were prescribed in a fluid ounce and half of camphor mixture at bed-time. Two hours' sound sleep were procured, and on awaking the patient felt the most satisfactory abatement of her sufferings. She was ordered to continue it in doses of eight minims, in combination with fifteen minims of the arsenical solution, every third hour during the day, and to repeat the full dose of thirty minims at bed-time; whilst at the same time the bowels were kept moderately lax. The most gratifying results followed this plan of proceeding; the pain gradually yielded; in less than a week she had comfortable nights, after which the dose of the solution was rapidly diminished, its use discontinued during the day, and on the third week I had the satisfaction of leaving her perfectly free from complaint.

"The second case was one of wakefulness without any apparent cause. All the usual preparations of opium had been tried without much benefit, and with suffering from headach and nausea on the mornings following the nights on which the narcotics had been given. The solution of the bi-meconate of morphia was given in doses of twenty minims; it effectually procured sleep, and was not productive of the morning distress which had supervened on the use of the other preparations of opium.

"The third case was one of anomalous pain of the hip extending down the thigh, which recurred three or four times in twenty-four hours. The patient had been frequently attacked with rheumatism: and conceiving it to be connected with that disease, the part was cupped, and the guaiacum mixture with small doses of blue pill was prescribed. Little benefit resulted until a week afterwards, when I ordered the part to be blistered, and the denuded part to be dressed with a piece of lint dipped in your solution thrice a-day. The pain rapidly abated, and on the fourth day it was completely gone.

"From the limited experience which I have had of the use of the solution, I am of opinion that it possesses decided anodyne properties, and stimulates less than opium or its tincture; and is much more certain in its influence than any of the artificial salts or other preparations of the day."

Cissampeline, a new vegetable base.—By A. Wiggers. (*Journal de Pharmacie,* January 1839.)—M. Wiggers announces that he has discovered in the root of *Cissampelos Pareira,* a vegetable basis, which is obtained in the following manner. The root is boiled in successive quantities of water acidulated with sulphuric acid, and carbonate of soda is added to the brown decoction. A precipitate is thus thrown down of a grayish-brown colour, which is washed and redissolved in water acidulated with sulphuric acid. The solution is boiled with animal charcoal, and after filtration carbonate of soda is added. A dirty yellow precipitate falls, which is dried, pulverized, and then boiled with ether. A nearly colourless solution is obtained, and by distilling off the ether, the cissampeline is procured. To purify the substance completely, it is dissolved in weak acetic acid, and again thrown down by carbonate of soda whilst the solution is still warm; the precipitate is then washed and dried with care.

It is known that M. Feneulle, who is occupied with the analysis of the *Pareira brava,* has discovered the presence of a yellow bitter principle in which the active properties of the root seems to reside. Can this principle be the impure cissampeline?

On the preparation of Gentianine.—by Prof. Dulk de Konisberg.—(*Journal de Pharmacie,* December 1838.)—The experiments of Tromsdorff and Leconte have demonstrated in a decisive manner that the gentianine prepared according to the directions of M. Henri, cannot be regarded as the active principle of gentian. I have found that the active bitter principle of this root may be isolated by the following process. Treat the coarse powder of the root with alcohol; distil off the alcohol, and dissolve the residue in water; filter the solution. The undissolved matter treated with ether furnishes a clear tincture, from which we obtain by evaporation the gentianine of M. Henri, quite insipid.

The aqueous solution has a very bitter taste, and must be fermented to separate the sugar, which cannot be easily done in any other manner. The liquid is then precipitated by the neutral acetate of lead; the precipitate is separated and thrown away. Into the bitter filtered liquid is poured the solution of the subacetate of lead, and a little ammonia to precipitate the combination of vegetable matter with the oxide of lead; but care must be taken not to add too much ammonia, because this, as the stronger base, is apt to separate the vegetable matter from the oxide of lead. We thus obtain a yellow precipitate, which is washed with small quantities of water, for a large quantity decomposes the compound. The precipitate is then suspended in water, and decomposed by a stream of sulphuretted hydrogen gas. Filter and evaporate at a low temperature to dryness; treat the residue with alcohol of specific gravity 0.820, filter, and by evaporation is obtained a mass which contains no traces of crystallization.

This gentianine has a yellowish-brown colour; dried and triturated, it furnishes a yellow powder; it possesses the bitter taste of the root in a high degree. It is hygrometric, is almost insoluble in absolute alcohol, more soluble in common spirit, and very soluble in water. It reddens litmus; heated it melts, swells up, and burns without residue; it contains no azote. By its reaction and relation to bases it approaches the acids.

SURGICAL PATHOLOGY.

Case of perfect Ankylosis of the five superior cervical vertebræ to each other, and complete dislocation backwards of the 5th from the 6th, without fracture.—By STEPHEN S. STANLEY, Esq. Assistant-Surgeon, Royal Hospital, Haslar.—George Weldon, aged 37, seaman, had for some years past been subject to a stiff-neck, frequently attended with rheumatic pains in that region, and subject to sore throat. He was nevertheless an active seaman, always doing his duty, and never on the sick list; but was unable to turn his head to either side, being compelled to move his whole body round when he wished to look either to the right or to the left. On the evening of the 20th July 1838, when engaged in some rough play with some of his messmates, his foot slipped, and he fell backwards, his head only slightly striking the deck. He complained immediately afterwards of severe pain in the back part of his neck, and between the shoulders, and of pain and numbness in the arms. His face was pale, and his pulse weak.

Next morning the numbness was complained of, not only in the arms, but also in the legs; the pain in the back parts of the neck was more severe, and he was unable to turn or move in any direction. No wound or external appearance of bruise was visible. Both arms were paralytic, the left most so. Since the accident he had not passed urine or feces; the pulse was slow, weak, and oppressed. The pupils of the eyes were not affected, and his breathing was natural. The urine was drawn off by the catheter; he had a draught of senna and an enema. In the evening his pulse had risen and was sharp; his bowels had been freely opened. Another injection was ordered, and twenty ounces of blood were drawn.

22d July. The respiration was hurried; pulse was weak; there was continued ineffectual attempts to expectorate frothy mucus; he was most anxious to inhale fresh air, and desired to be raised high in bed. The catheter was again used, the enema repeated, and a cathartic bolus administered. At noon he was easier; the breathing was more free; but the pulse flagged. Towards evening the breathing became more laborious, and he gradually sunk. He died at half-past four o'clock on the morning of the 23d, exactly fifty-five hours and a-half after the accident.

Post Mortem Appearances.—On the posterior surface of the body, from the occiput to the sixth or seventh dorsal vertebræ, there was considerable ecchymosis; and in cutting through the skin and subcutaneous cellular tissue a quantity of blood was observed effused into its texture. In a space reaching from the first cervical to the second dorsal vertebræ, coagulated blood in great quantity was found surrounding the muscular fibres, a number of which were ruptured and softened. These being dissected back exposed to view a considerable displacement backwards of the fifth from the sixth cervical vertebræ. Upon the parts being carefully cleaned it was found that the little finger could be easily passed underneath the dislocated vertebræ into the spinal canal; that the body of the fifth vertebra pressed severely on the spinal cord, and rested on the laminæ and spinous processes (? transverse) of the sixth cervical vertebræ. The spinal column was then carefully removed, and it was ascertained beyond all doubt, that the injury was a complete dislocation *without fracture.* The ligaments and intervertebral substance were all ruptured; and when suspended from above, the parts were held together solely by the vertebral arteries and spinal marrow, with its theca, the *theca vertebralis* being uninjured.

The cranium was thick and very heavy. The longitudinal sinus was gorged with blood; and the medullary substance of the brain was soft and very vascular. On removal of the brain it was found that the *foramen magnum* of the occipital bone was so contracted as scarcely to admit the point of the little finger. This was discovered to be owing to the odontoid process of the second cervical vertebra being much larger than natural, and projecting upwards towards the base of the brain, and backwards on the *medulla oblongata*. The parts being removed and dissected, it was ascertained that the whole of the cervical vertebræ, from the atlas to the seat of the dislocation, were completely ankylosed. Not the least vestige of ligamentous structure could be observed, with the exception of the capsular and occipito-atlantal ligaments, forming the articulation between the occiput and atlas. The capsular ligaments and synovial membranes, when cut into, were found to be so much thickened and altered in structure, that they bore a strong resemblance to cartilage. No trace was found of the *apparatus ligamentosus* and lateral ligaments which connect the occiput with the atlas; neither was there any thing in the form of ligaments which complete the articulation between the atlas and the axis. The absence of the transverse ligament was supplied by an isthmus of bone extending from the anterior aspect of the odontoid process to the posterior concave surface of the anterior arch of the atlas. This bridge of bone was half an inch in length, and varied from three to four lines in breadth. The most remarkable feature in the whole preparation, and evidently the result of a former dislocation forwards, was the position of the atlas; which, on the right side especially, was pushed forwards and upwards from off the articulating surface of the axis, so as to cause the odontoid process to present itself nearly in the centre of the circle of the atlas. The axis is also pushed forwards in the same manner from the third cervical vertebra, but not to so great an extent, giving the preparation a twisted appearance to the left side. The diameter of the spinal foramen of the atlas, from behind forwards, was exactly one inch four lines; and the transverse diameter one inch and half a line. The odontoid process, instead of terminating at its apex in a point, as it generally does, presented a broad and irregular ovoid form, measuring transversely half an inch; and from behind forwards, including the bony bridge alluded to, one inch; its length was three-fourths of an inch, and its distance from the posterior arch of the ring of the atlas only four lines.

MISCELLANEOUS.

Harveian Society. Notice to Students of Medicine.—The Harveian Society of Edinburgh have fixed on the following subject for their Prize Essay for the year 1840, viz.:—"*The Diagnosis of Disease, as founded on the conditions of the Saliva and Expectoration.*"

Dissertations on this subject must be transmitted to one of the Secretaries, on or before the 1st day of January 1840. Each Dissertation must be accompanied by a Sealed Letter, containing the Name and Address of the Author, and inscribed on the back with a motto. The same Motto must also be prefixed to the Dissertation to which the Letter belongs. None of the Sealed Letters are ever opened, except that bearing the same Motto with the successful Essay.

The Prize given by the Society, to the successful Candidate, is an elegant Silver Medal, with a suitable Inscription.

The Candidate is at liberty to employ his Essay afterwards, in any way he may think proper: And, accordingly, some Dissertations submitted to the Harveian Society, have, in consequence of publication, been productive of considerable honour and emolument to the Authors.

By order of the Society,

Richard Huie, M. D. 8, George Square, } *Secretaries.*
P. D. Handyside, M. D. 10, Shandwick Place, }

Edinburgh, *March* 12, 1839.

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